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Journal of Experimental Psychology

EDITED BY

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INVOLUNTARY MOTIONS OF THE EYE DURING MONOCULAR FIXATION *

BY FLOYD RATLIFF AND LORRIN A. RIGGS

Brown University

INTRODUCTION

The eyes are continually in motion, even when an attempt is made to maintain steady fixation. These small involuntary motions of the eye during fixation are sometimes referred to as physiological nystagmus.

The general purpose of this experiment is to measure the physiological nystagmus during monocular fixation of a stationary object. Both vertical and horizontal components of the movements are recorded simultaneously by means of a photographic method. The extents, frequencies, and directions of the movements are determined by measurements of the photographic records. On the basis of the data obtained from these records conclusions are drawn regarding the motions of the retinal image of the object being fixated.

The significance of this problem lies in the fact that great difficulties are encountered when an attempt is made to explain the observed facts of visual acuity solely on the basis of the physical properties of light, the optical properties of the eye, and the dimensions of the retinal receptors.

Recently, several investigators have considered the possible importance of physiological nystagmus in determining the limits of visual acuity. However, no clear relationship between physiological nystagmus and visual acuity has been demonstrated experimentally. (Experiments designed to determine whether or not such a relationship exists are now in progress in this laboratory.)

Aside from the acuity problem, physiological nystagmus is an important factor in relation to the possibility of microstimulation of retinal receptors. It is evident that single receptors or very small areas of the retina of the living human subject cannot be stimulated alone unless the eye movements are of negligible size or unless some method is designed to circumvent the effects of the motions.

HISTORY OF THE PROBLEM

The existence of physiological nystagmus was suspected long before there was any objective experimental evidence of

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it. Jurin (8), in 1738, noted that the visual angle must be greater to distinguish two marks as separate from one another than to recognize a single mark by itself. His suggested explanation was that when the eye trembles two separate images merge together, while a single image may continue to be seen in one location after another.

Helmholtz (6), about 1860, recognized the fact that the maintenance of perfect fixation is extremely difficult. He supposed that this "wandering of the gaze" served to subject all parts of the retina to alternating strong and weak stimulation, thus preventing the development of retinal fatigue. Since Helmholtz several different techniques of recording these movements have been devised.

A search of the literature reveals that E. B. Huey (7), about 1899, was one of the first experimenters to record this wavering of fixation by objective methods. Huey's apparatus consisted mainly of a very light stylus recording apparatus attached to a cup capping the cornea. The apparatus was designed to record eye movements on a smoked drum during reading, but was later slightly modified to measure reaction times. Huey found that he had to deduct a small amount from the measured reaction time because of a tremulous motion of the recording pointer which occurred while the subject was maintaining what he thought was an absolute fixation. It was evident that some motion of the eye was occurring even during 'steady fixation.'

Delabarre (2), 1897–98, independently devised a method quite similar to the one used by Huey. However, Delabarre apparently did not publish any of the results which he obtained using this method.

In 1898 Orschansky (13) devised a photographic method of recording eye movements. The attachment to the eye consisted of a metal or glass shell made to fit over the cornea. There was a small opening in the shell for the pupil and a small mirror was attached to the side of the shell. Records of movements could be obtained by reflecting a light from a small source on to a moving film by means of the mirror. Apparently Orschansky did not publish any of the results of his method either, but the method alone merits some attention since it was the forerunner of several methods of similar nature, including the one used in the present experiment.

In 1901 Dodge and Cline (3) devised a photographic method of recording eye movements which did not require the use of attachments to the eye. Originally the method consisted of photographing the eye itself by means of a slowly moving film. Later the method was modified and instead of photographing the eye directly, they photographed a bright vertical line reflected from the cornea. Dodge and Cline did not investigate eye movements during fixation at this time, but their method was used by other investigators.

McAllister (10), in 1905, investigated the behavior of the eye when an observer is attempting to fixate a point. The method used was similar to that used by Dodge and Cline. However, since the bright spot on the cornea photographed by Dodge and Cline does not move through exactly the same path as the surface of the cornea, some modification was necessary. Instead of using light reflected from the cornea McAllister photographed a fine flake of chinese white adhering to the cornea. One of the conclusions drawn from the results of McAllister's experiment is that the image of a point fixated does not fall on any particular part of the retina, but may fall on any point of a considerable area of the retina. The angular diameter of the area of "wandering" of the image of a fixated point as reported by McAllister appears to be about one degree.

Dodge (4), in 1907, in addition to the methods described earlier, used an after image method for the study of visual fixation. He found that even the greatest effort to fixate some point does not keep an after image motionless. In his own case Dodge found the angular diameter of the area of wandering to be about 10 min. of arc, with occasional more variable movements of nearly 30 min. of arc. Dodge states that the main theoretical

value of the after image demonstration is the conclusive evidence which it furnishes that the rhythmic movements involve actual movements of the point of regard and not merely misinterpreted head movements.

In 1911 Marx and Trendelenburg (11) carried out what is probably one of the best of the early objective studies of physiological nystagmus. Their method was similar to the one devised by Orschansky. A thin aluminum shell was constructed to fit the cornea and sclera of the subject's eye. A small opening was cut in the center of the shell to provide an opening for the pupil. A small mirror was placed to one side of this opening. Light from a small source was reflected from the mirror and photographed on a moving film. Head movements were recorded simultaneously by means of a mirror attached to a pince-nez frame fastened securely to the nose. Head movements were controlled to a certain extent by means of head rests and a biting board. Only one subject was used. The main results of the experiment were as follows: During the fixation of a point the eye carried out constant large and small movements which were usually not greater than four to five and a half min. of arc in extent. In some cases the deviations from 'perfect' fixation exceeded the above values; yet they were always small enough for the image of the point to remain within the limits of the fovea. No significant difference was noted between the horizontal and vertical components of the eye movements. (These two components were not measured simultaneously since the apparatus was capable of measuring only one component at a time.) Head and eye movements did not seem to be closely related. Occasionally, sudden shifts of the eye occurred.

In 1934 Adler and Fliegelman (1) investigated the problem of eye movements during fixation. Their method was similar to that used by Marx and Trendelenburg, except that a small mirror was placed directly on the eye. The subject held the upper eyelid open with his finger in order to prevent involuntary lid closures. Light from a ribbon filament lamp passed through a lens and was reflected from the mirror, finally coming to focus on the moving film of an electrocardiographic camera. Apparently only one subject was used and only horizontal movements of the eye were recorded.

Adler and Fliegelman describe three types of movement which took place during the fixation of a cross hair on a white background. (1) Rapid shifts of the eye occurring at the rate of about one per sec. and involving an excursion of 12.5 to 17.5 min. of angular rotation. (2) 'Waves' occurring at the rate of five or more per sec. with excursions of 2.5 to 5 min. of angular rotation. (3) Fine vibratory movements, 50 to 100 per sec., having average excursions of two min. and fourteen sec. of angular rotation. In addition to the 'types' of motion described above, there was noted a tendency for the eye to gradually 'drift' in one direction or another.

In 1948 Lord and Wright (9) recorded eye movements during monocular fixation. Their method was as follows. A beam of ultraviolet radiation falls on the cornea from a source so placed that the light transmitted through the cornea falls on the subject's blind spot when he fixates his eye on the test object. Part of the ultraviolet light is reflected at the cornea, this reflected beam falls on a horizontal straight edge, and the other part on a vertical straight edge. As the eye moves, more or less of the radiation passes by the straight edges. The part of the radiation which passes the straight edges is, in each case, focused on to an electron multiplier photocell. The output of each photocell is fed to an amplifier and cathode ray oscilloscope, operating without a sweep circuit. The traces of the two oscilloscope beams are photographed by means of a continuously moving film. Horizontal and vertical components of the eye movements may be deduced from the records on the film. The authors state that the apparatus is sufficiently sensitive to detect eye movements of one min. of arc. Deflections due to noise from the multipliers are said to be equivalent to movements of not

more than half a min. of arc, as a general rule. Head movements were recorded by means of an artificial cornea attached rigidly to the front teeth of the upper jaw.

These investigators state that the main feature which distinguishes the eye movements from the head movements is the rapid 'flick' lasting 0.02 to 0.03 sec. and indicating a momentary angular movement of the eye of amplitudes ranging from about three to fourteen min. of arc, and an average frequency of about two per sec. for one subject and three every two sec. for another subject. Lord and Wright state that between these movements most of the recorded movement could be attributed to head movements, although in the absence of simultaneous head and eye movement records this is not certain. The records suggest that after each 'flick' the eye may take up a slightly different fixation direction displaced some few min. of arc from the previous direction. After-image experiments seem to confirm this interpretation. In the majority of cases random movements of 10 or 20 min. of arc commonly occur. Tests made with various fixation targets showed no variation in fixation ability, relative to the day to day variation in ability to fixate a given target. Lord and Wright state that there is little evidence of any fine vibratory movements having a period of the order of 0.02 sec. as reported by Adler and Fliegelman.

O'Brien and O'Brien (12) in 1948 obtained results from an experiment which indicated that no movement of the eye greater than two min. of arc occurred during periods of $\frac{1}{5}$ sec. or less. A single star test object was presented for monocular observation in the center of a black disc subtending 15 min. of arc and surrounded by a large, white adapting field illuminated steadily. The star was illuminated by short bursts of 10-microsec. flashes. The frequency of the flash repetition within a burst was varied from 18 to 240 per sec., and the duration of a burst from $\frac{1}{5}$ to 1/100 sec. In no case were multiple star images visible, from which finding the authors draw the conclusion that no ocular movement as great as two min. of arc occurred during a burst of flashes.

Hartridge and Thomson (5), in 1948, used a greatly improved corneal reflection method to measure eye movements during fixation. A cine camera capable of taking up to 60 frames a second photographed a magnified image of a light source reflected from the cornea as well as two images of a reference source. The fine definition and absence of blur in the enlarged image of the corneal reflection led these investigators to the conclusion that the eye could not be performing rapid vibratory movements.

This summary of the studies of physiological nystagmus indicates that there is recent experimental evidence to support three different positions with regard to rapid vibratory motions, as follows: (1) The eye is in motion during fixation, and among the motions are fine vibratory movements averaging in number from five to ten in 0.1 sec. and having average angular extents of slightly more than two min. of arc (Adler and Fliegelman). (2) The eye moves during fixation, but there is little evidence of fine vibratory motions having a period of the order of 0.02 sec. (Lord and Wright). (3) The eye is essentially motionless during short periods of fixation (Hartridge and Thomson).

Experimental Section

Purpose of Experiment

The primary purpose of the present experiments is to resolve the conflict in the above evidence on the existence of 'fine vibratory eye movements' during monocular fixation of a stationary test object. To this end it has been necessary to devise apparatus which will make photographic records of even the most minute of these movements under conditions as comfortable as possible for the subject. The results are interpretable in terms of

their contribution to visual acuity under optimal conditions of fixation.

Apparatus

A photographic method of recording was used. The basic elements of the recording apparatus are shown in Fig. 1. A source of light was placed at a point in the focal plane of a lens so that the rays from the source became parallel when they passed through the lens. These parallel rays were reflected from a plane mirror and passed back through the same lens. The rays then came to a focus on a moving strip of film in the focal plane of the lens. Any movement of the plane mirror resulted in a proportional displacement of the image of the light source on the moving film.

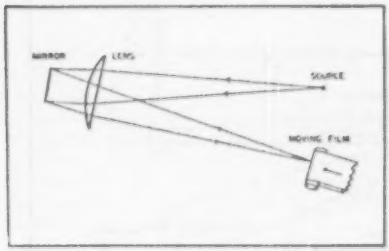

FIG. 1. Basic elements of the recording system

The contact lens arrangement

It is obvious that the cornea itself could not be used for the reflecting surface with this type of apparatus, since it does not have a plane surface. The best solution to the problem of providing the necessary plane surface seemed to be a modification of the methods used by some of the earlier investigators in which a cup-shaped attachment with a mirror on its outer surface was fitted to the eye. The modern contact lens, now in widespread use, fits the requirements quite well. Each of the subjects was individually fitted[1] with a contact lens for the right eye. A hole approximately five mm. in diameter was drilled through each contact lens just slightly on the temporal side of the corneal bulge. A good quality first-surface mirror was then fitted tightly into the hole in the lens, and cemented in place by means of airplane glue. Great care was taken to insure that the inner part of the lens which came in contact with the eye was perfectly smooth. Soft wax was placed around the outer edge of the mirror to prevent irritation to the lids and possible leakage of the contact lens solution.

[1] We are indebted to Dr. E. V. Troendle for his skill in fitting these lenses.

Apparatus for photographic recording

The basic elements of the apparatus as shown in Fig. 1 required some modification in order to permit the simultaneous recording of horizontal and vertical components. A diagram of the completed apparatus is shown in Fig. 2. All of the parts of the optical system, except those included in the oscillograph, were fastened securely to a very heavy steel 'I'-beam. A biting board for the subject was also attached to this beam. A straight wire filament lamp in a horizontal position was used as a source of light. Between this lamp and the main lens of the apparatus was placed a dove prism rotated 45° from its 'normal' position. This prism was placed slightly to one side of the line from the light source to the center of the main lens. The effect of the prism was to rotate part of the light from the filament through an angle of 90° so that there were, in effect, two apparent sources of light, a horizontal filament and a vertical filament. The focal length of the lens was 1000 mm. Since the source of light was in the focal plane of the lens, the rays from any point at the source were rendered parallel by the lens. The parallel rays struck the mirror in the contact lens being worn by the subject and were reflected back through the main lens. The rays from the apparent vertical filament provided by the rotated dove prism, after being reflected back through the main lens were brought to a focus on a vertically moving strip of film by means of a cylindrical lens. The rays from the horizontal filament, after being reflected back through the main lens, passed through a dove prism rotated 45° so that the image of the filament would be vertical when it reached the cylindrical lens. Each bundle of rays was then brought to a point focus on the moving film by means of the cylindrical lens.

The above arrangement was such that any vertical movement of the eye would not alter the position of the image of the vertical filament on the film, but would cause the image of the horizontal filament to be displaced on the moving film. Conversely, any horizontal movement of the eye would not alter the position of the image of the horizontal filament on the film, but would cause the image of the vertical filament to be displaced on the film. Any oblique movement of the eye would cause both images of the filament to be displaced on the film, each by an amount proportional to the amount of the respective horizontal and vertical components of the eye movement.

A Hathaway oscillograph with the plate removed from the galvanometer end of the case

provided the necessary cylindrical lens and camera. During most of the experiment 35 mm. or 60 mm. Kodak Cardiographic Paper No. 1115 was used in the oscillograph camera. For some of the control records 35 mm. Ansco Ultra Speed film and Kodak Micro-File film were used. A synchronous motor, with a device to close a circuit every 0.1 sec. and deflect a galvanometer in the oscillograph, was used as a time marker.

Fixation test objects

The subject's task was to fixate steadily on a test object appearing at the center of a white paper disc. A special microscope lamp was used to illuminate this test object. A movable carrier for the fixation point was provided by a vertical rack and pinion, the base of which was fastened to the steel beam, and a horizontal bar along which a plate bearing the test object could be moved. This two-dimensional manipulation of the fixation point made it possible to bring the eye into position in such a way that the mirror in the contact lens reflected the images of the filament to the proper positions on the photographic film.

Head movements

Records of head movements were obtained by means of a mirror attached to the head. A pair of goggles with the glass removed provided a support for this mirror. The head mirror was adjusted to a position quite near the eye mirror. With this arrangement the recording apparatus described above was capable of utilizing both mirrors to make a simultaneous record of horizontal and vertical components of both head and eye movements.

Originally it was planned to use the simultaneous records of head and eye movements to determine the exact motions of the retinal image, because in this experimental situation both the head and eye movements must be known to determine the motions of the image on the retina. However, it became apparent during the experiment that the center of rotation of the head is not even approximately a fixed point. At times this center of rotation may be at the point of contact of the upper teeth with the biting board. At other times it may be at the point of articulation of the vertebral column with the cranium. Since the center of rotation of the head cannot

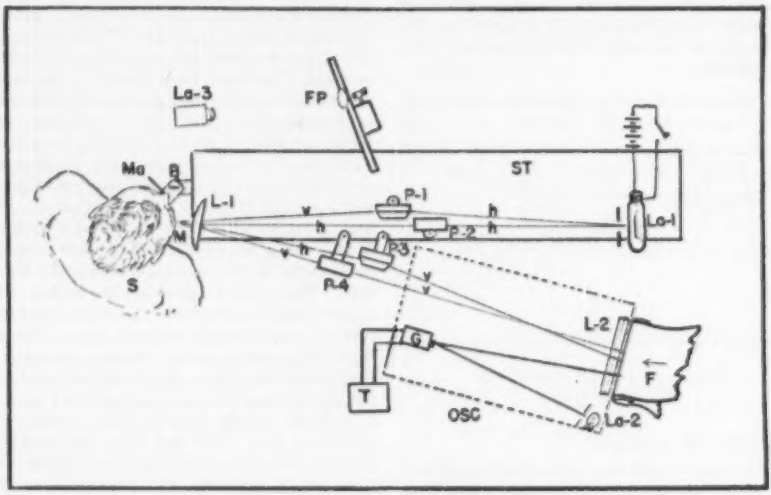

FIG. 2. Diagram of the recording apparatus. B: Biting board. F: Moving film. FP: Fixation point. G: Galvanometer. h and v: h indicates that image of the filament would be horizontal if the rays were brought to a focus at this point, v indicates that the image would be vertical. La-1: Source of light for the recording system. La-2: Oscillograph lamp. La-3: Microscope lamp. L-1: Main lens of recording system. L-2: Cylindrical camera lens. M: Mirror in contact lens. Ma: Mask covering left eye. OSC: Oscillograph. P-1 and P-3: Dove prisms rotated 45° from 'normal' position. P-2 and P-4: Dove prisms in 'normal' position. S: Subject. ST: Steel 'I' beam. T: Timing device.

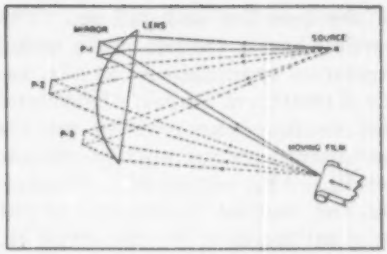

FIG. 3. The effect of displacement of the mirror while it remains in a given plane, or in a plane parallel to a given plane. P-1: Original position of the mirror. P-2: Mirror in a plane parallel to the plane of the original position. P-3: Mirror in the same plane as the original position of the mirror.

be determined with accuracy, the angular movements which are recorded are of little use in determining the linear extent of the movement of the eye due to head movements. Because of this objection a better method of determining the movements of the retinal image was needed. The apparatus described above required only a slight modification to overcome the problems introduced by head movements.

It may be shown (see Fig. 3) that any horizontal or vertical displacements of the mirror in the recording apparatus will not alter the position of the image of the light source on the recording film *so long as the mirror remains in the same plane.* It may also be shown that any displacement of the mirror away from or towards the main lens of the apparatus will not alter the position of the image on the film *so long as the mirror remains in a plane parallel to the plane of its original position.* Thus, any combination of the movements described above will not change the position of the image of the light source on the film so long as the mirror remains in a given plane or in a plane parallel to the given plane. In this experimental situation the mirror on the contact lens will remain in a given plane, or in a plane parallel to the given plane, *if the subject maintains perfect fixation on a stationary object at an infinite distance.* Under these conditions any movements which are recorded will, therefore, represent deviations from perfect fixation. That is, head and eye movements are not recorded unless they cause deviations from perfect fixation. With such an arrangement motions of the retinal image may be computed without the necessity of separate determinations of effects of head and eye movements. The only modification of the apparatus necessary to provide the above conditions was to provide a fixation point at an infinite distance. It was not practicable to set up and manipulate a fixation point at a distance from the eye which was great enough to be considered infinite. Hence it was necessary to provide a fixation point which was optically at an infinite distance. This was accomplished by placing a collimator between the subject's eye and the fixation point. With this arrangement all light rays from any point being fixated were parallel when they reached the subject's eye, just as they would be if the object being fixated were at an infinite distance.

Procedure

Records were first obtained during fixation of a stationary black point on a white background having a brightness of approximately 60 footlamberts. The fixation point was approximately 500 mm. from the subject's eye and subtended a visual angle of about 3.45 min. The white background subtended a visual angle of about 10 degrees. The subject fixated the dot with his right eye while the experimenter manipulated its position to bring the reflected images into the proper position in the recording apparatus. The left eye was covered by a paper mask. The subject was then instructed to maintain steady fixation, and a record about two to four seconds long was made of the eye movements. The subject was given short rests between the fixation periods, and the experiment was discontinued whenever the subject showed signs of fatigue. Eye movement records were obtained from five male subjects ranging in age from 20 to 36 years. All had 'normal' (20/20 or better) Snellen Chart acuity. Simultaneous head and eye movement records were obtained from two of the subjects by means of the head mirror described above. The collimator to provide a fixation point at an optically infinite distance was not used in this part of the procedure.

Records were next obtained from three of the subjects during the fixation of a stationary cross. Then records were obtained from the same three subjects during the fixation of a stationary point. In these two cases the collimator described above was placed between the subject's eye and the test object. Hence, the fixation point was optically at an infinite distance from the subject's eye. The procedure was essentially the same as described above, except for the test objects. Here the test objects were a dot, whose diameter subtended a visual angle of 2.6 min., and a cross made of lines having a width subtending 1.2 min. of visual angle. The apparent brightness was reduced by the collimator to a value of about 45 footlamberts. The extents from peak to trough of the wave-like records were measured by means of a microscope and a slide graduated in

tenths of a mm. From these measurements the angle of rotation of the eye could be computed to an accuracy of about 10 sec. of arc.

In the case of the recording apparatus used in this experiment it can be shown that the eye may be considered as rotating about a point at the optical center of the main lens. Since the mirror is parallel to a vertical axis through Listing's plane, the tangent of the angle of rotation of the eye in the horizontal plane may be computed by dividing one-half the amount of displacement of the corresponding trace by the distance from the main lens to the film. (It is necessary to use one-half the displacement because the light beam reflected by the mirror turns through twice the angle of rotation of the mirror.) For the vertical component a slight error is introduced by the fact that the surface of the mirror is not parallel to the horizontal axis through Listing's plane. This has the effect that only about 85 percent of the vertical component (motion about a horizontal axis through Listing's plane) is shown by the corresponding trace on our records.

Results

Samples of records from one of the subjects are shown in Fig. 4. The records are to be read from left to right. The time line on the base of each record is displaced every tenth of a sec., with a fairly long displacement of the time line each full sec. The vertical line at the left of the record represents approximately 20 min. angle of rotation of the eye. Horizontal and vertical components of the eye movements are indicated by the letters E_h and E_v, respectively. Horizontal and vertical movements of the head are indicated by the letters H_h and H_v, respectively. Displacement of the vertical component towards the time line results from an upward movement of the eye. Displacement of the horizontal component towards the time line results from a movement of the eye towards the subject's right.

Two outstanding features of these records of eye movements are (1) the sudden jerks, and (2) the small rapid motions. Two other 'types' of motion can be seen also; relatively slow motions of irregular frequency and extent, and drifts in one direction or another. At times the drifts appear to be compensated for by the rapid jerks. A general description of the eye movements during fixation, based on all of

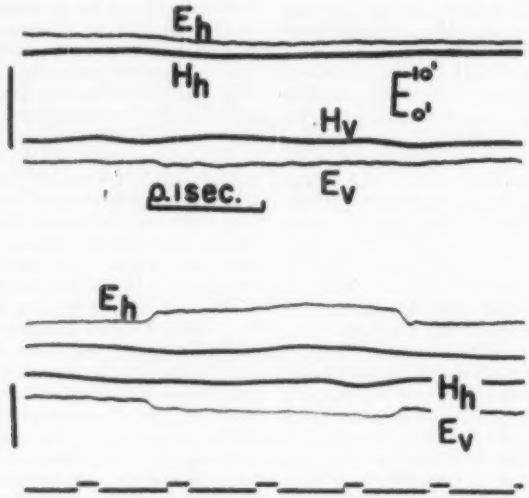

FIG. 4. Samples of head and eye movement records. (See text.)

the records obtained from all five subjects, is given below.[3]

The small rapid motions

These motions had frequencies of from 30 to 70 cycles per sec. In a few cases the frequencies were slightly greater than 70 cycles per sec., but for the most part they fell within the range indicated. The frequencies are not constant over long periods of time, but may vary considerably from one section of a record to another. The median extent of these motions (peak to trough) in both components for the five subjects was 17.5 sec. of arc. The total extents of the motions ranged from those which were just perceptible to those of almost two min. of arc. However, rapid motions with total excursions greater than one min. of arc were quite rate.

The relatively slow motions

Relatively large slow motions occurred which had no characteristic frequency or extent. These motions are somewhat different from one subject to the next, and there is considerable variation within the records of individual subjects. The motions are complex and defy simple description. However, they may be roughly described as having total extents (peak to trough) ranging from one to five min. of arc, and frequencies of from two to five cycles per sec.

Slow drifts

These slow drifts, upon which the motions mentioned above are superimposed, may be described as a gradual shifting of the wave form in one direction or the other. The extent of these drifts usually did not exceed five min. of arc. The drifts were found in records obtained from all five of the subjects.

Rapid jerks

These motions occurred at irregular intervals. Occasionally they were not more than 0.2 sec. apart, while in some cases no jerks occurred for a period of three or four sec. The average extent of these motions, based on the records obtained from all five subjects, was 5.6 min. of arc. The range of the extent of the motions was from 2.2 to 25.8 minutes of arc. However, jerky motions with extents greater than 10 min. of arc were not common. At times these jerks seemed to compensate for the drifts, but they often occurred when no drifting had taken place.

Total movement

The total movement during a period of three or four sec., due to the combined effects of the motions described above, was usually less than 10 min. angle of rotation during the steadiest fixations. However, in some cases the total movement was as great as 20 or 30 min. angle of rotation.

The head movements

In some cases the head and eye movements are related, but only as far as the slower motions and the general shape of the wave form are concerned. However, the records of the head movements show no evidence of the small rapid motions or of the rapid jerks which are the outstanding features of the eye movement records. The head movements were somewhat variable from one subject to the next and may account for some of the differences noted in the slower compo-

[3] The possibility of subjecting these records to a frequency analysis has been considered. It is believed, however, that this procedure would be less closely related to the visual acuity problem than the estimates in the discussion below concerning the visual receptor elements.

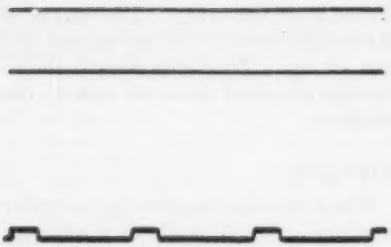

FIG. 5. Control record obtained with contact lens and mirror firmly attached to the biting board

nents of the eye movements of the various subjects.

The effect of different fixation objects

Records of eye movements obtained during fixation of a cross do not appear to differ from those obtained during the fixation of a point. Careful measurements of these records indicate that, within the limits of this experiment, the shape of the fixation object does not have any effect on the nature of the physiological nystagmus.

Control records

Two types of control record were needed: (1) records to insure that none of the movements recorded were artifacts of the recording system, and (2) records to determine whether or not the contact lens was following the movements of the eye during fixation. Control records to detect possible mechanical artifacts in the recording system were obtained as follows. A contact lens with mirror was firmly attached to a mount on the subject's biting board. The position of the lens was adjusted so that the images of the light source fell on the film. The recording apparatus was then operated in the usual way. A sample of the records obtained by this procedure may be seen in Fig. 5. No deviations can be seen in the traces which resemble the deviations in the records of the head and eye movements. Evidence that the small rapid motions and jerks are not artifacts of the recording apparatus is also to be found in the simultaneous records of head and eye movements (Fig. 4). In these records the traces of the head movements show no signs of the small rapid motions or jerks which are characteristic of the eye movements. If these irregularities in the traces were artifacts of the recording system, they would be expected to appear in traces from the head as well as those from the eye.

Control records to determine whether or not the contact lens follows the eye movements were obtained as follows. A small disc about six mm. in diameter was cut from a piece of absorbent tissue paper. A small amount of wax was melted and dropped on the center of the disc, permeating the central part of the paper. A thin first surface plane mirror about two mm. in diameter was imbedded in the wax. When the wax had hardened the paper disc was moistened slightly and placed directly on the eye of the subject, just at the temporal side of the cornea. The absorbent tissue adhered to the eye perfectly. It was necessary to tape the lids apart to prevent involuntary lid closures. No drugs were required, since there was but little discomfort as long as the cornea was not touched. There was considerable flowing of tears, however, after the lids had ben taped apart for a few minutes. Presumably this was due to the drying of the cornea and sclera because they were no longer in contact with the moist lids. With the above modification of the apparatus short series of records could be obtained in the usual manner.

Samples of records obtained by use of the usual contact lens and mirror and by means of a mirror placed directly on the eye are shown in Fig. 6. These records are quite similar. Slow waves, small rapid waves, and a jerky motion may be seen in both records. On the basis of the similarity of these two sets of records it seems reasonable to assume that the contact lens follows the eye movements with a fair degree of accuracy.

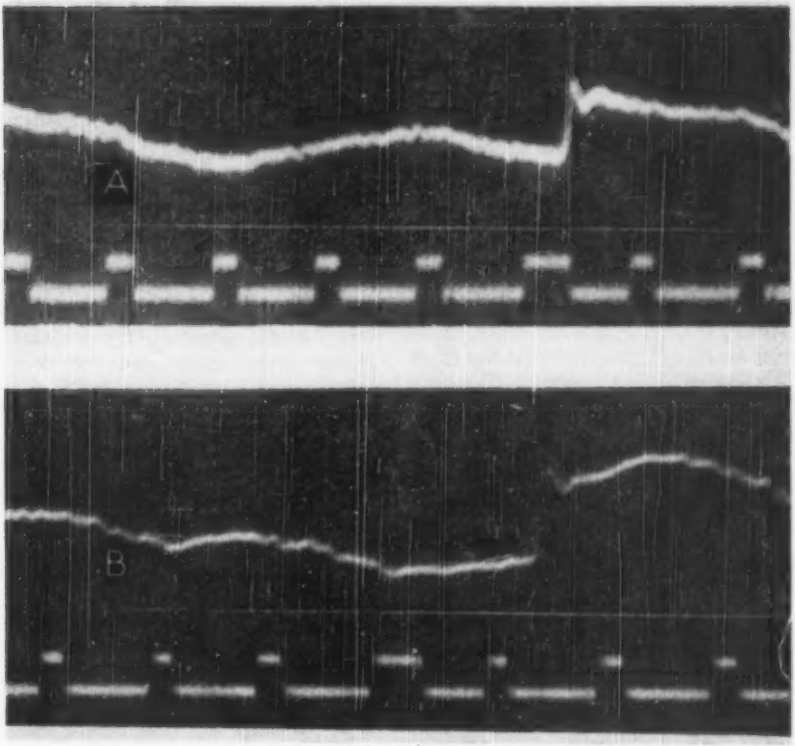

FIG. 6. Control-record. Record A obtained with a mirror placed directly on the eye. Record B obtained with mirror mounted on contact lens.

DISCUSSION

Motion of the Retinal Image

On the basis of the records obtained in this experiment it is possible to draw conclusions regarding the motions of the retinal image of an object during fixation. The treatment of the eye as a refracting system may be simplified if it is assumed to have a single principal nodal point. Since this nodal point is not at the center of rotation of the eye, the visual angle subtended by the displacement of the retinal image is not exactly equal to the angle of rotation of the eye. However, Sundberg (15) has shown that if the fixation point is at a considerable distance from the eye, the angle of rotation and the visual angle subtended by the displacement of the retinal image may be considered equal. This point is illustrated in Fig. 7. Assuming these two angles to be equal introduces a slight error when computing the extent of the motion of the retinal image. The error is negligible, however, except for large eye movements occurring when the fixation point is very near the eye.

Movements of retinal images of objects being fixated may be stated in terms of the number of receptors over which a given contour may be ex-

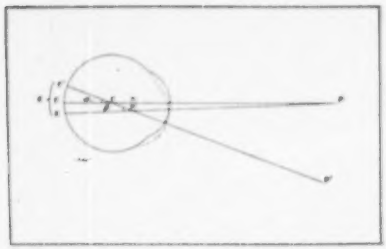

FIG. 7. The effect of the displacement of the nodal point on the position of the retinal image. If the eye turns through the angle Alpha about the center of rotation (c) so that the visual axis moves from P to P′ the part of the retina (F) on which the image of P had previously fallen moves to a new position (F′). However, because the nodal point (n) moves to a new position (n′), the image of P now falls on the retina at the point X. The total displacement (d) of the retinal image of P is the distance which subtends the visual angle Beta. If P is at a considerable distance from the eye, the lines FP and XP may be considered parallel. If these lines are parallel the angles Alpha and Beta must be equal. Therefore, under these conditions the angle of rotation of the eye and the visual angle subtended by the displacement of the retinal image may be considered equal. The following dimensions of the eye were used in all computations: Fc equals 11 mm.; cn equals 5.92 mm.

pected to pass. Polyak (**14**) describes the dimensions of the receptors as follows:

> The thinnest cones are in the very center of the fovea, where their thickness, in Man, is reduced to almost one mu, corresponding to a visual angle of approximately 12″ to 15″ of arc. The central territory where the cones are almost uniformly thick measures approximately 100 mu across, corresponding to 20′, or one third of a degree, of arc. It contains approximately 50 cones in a line. This area seems to be not exactly circular, but elliptical, with the long axis horizontal, and may contain altogether 2000 cones. This territory, accordingly, represents the peak of the structural perfection of the eye. If one considers that here practically every cone is individually linked with the ganglion cells by means of its own "private" midget bipolar and that accordingly, in this sense it represents an independent functional unit—at least in so far as the "pure cone system" is concerned, it is permissible to conclude that in this central territory the number of visual units corresponds pretty well with the number of cones. In consequence, the size of each of the 2000 receptor-conductor units measures, on the average, 24″ of arc. The size of the units even in this territory varies, however, the centralmost measuring scarcely more than 20″ of arc or even less. Of these—the most reduced cones, and therefore the smallest functional receptor units—there are only a few, perhaps not more than one or two dozen. The size of the units, as given, includes the intervening insulating sheaths separating the adjoining cones from one another.

The amount of angular rotation of the eye during fixation, and hence, the visual angle subtended by the motion of the retinal image, may be roughly summarized as follows: Total movement over a period of three to four sec. is 10 to 20 min. of arc. Total extents of slow movements, drifts, and jerks are approximately five min. of arc. Total extent of small rapid motions: rarely greater than one min. of arc, median value approximately 18 sec.

In terms of the number of receptors across which the retinal image moves, these motions of the eye may be described as follows: over a period of three to four sec. the retinal image may still remain within the limits of the most sensitive area of the fovea. Assuming the average diameter of the cones to be 24 sec., as Polyak has stated, then such movements would have carried the retinal image of the object fixated across a total of 25 to 50 receptors. The slower motions, drifts, and jerks would carry the retinal image of an object fixated across about a dozen receptors.

The small rapid motions are of particular interest, since they appear to represent an irreducible minimum of eye movement such as would characterize optimal fixation on an acuity test object. Since the small rapid motions rarely exceed one min. of arc, movement of the retinal image because of these motions cannot exceed the

width of two or three receptors. In fact, the median value of these motions is even less than the width of a single receptor in this area, except for the very smallest cones, of which there are said to be only one or two dozen. However, because of the other motions which are occurring, it appears unlikely that any point of an image can remain on a single receptor for more than a few hundredths of a sec.

A Comparison with Previous Experiments

The results obtained by Marx and Trendelenburg, Adler and Fliegelman, and Lord and Wright all support the general conclusion of this experiment that the eye is never motionless, even during the 'best' fixations. On the other hand, Hartridge and Thomson have obtained results which indicate that the eye is essentially motionless during fixation.

There is little agreement among the experimenters who report eye movements during fixation regarding the nature, or even the existence, of the small rapid motions. Records obtained by Marx and Trendelenburg do not show any evidence of these small motions. However, the speed at which their recording paper moved was so slow that even if such movements occurred it is unlikely that they could be detected in their records.

The records obtained by Adler and Fliegelman indicate that small motions of high frequency do exist. However, the extents of the motions which they recorded (*mean* values of 2 min. 14 sec.) are much greater than the extents of the small rapid motions recorded in this experiment (*median* value of 17.5 sec.). Since they apparently used only one subject it is quite possible that individual differences may account for some of the discrepancy, but the individual differences noted in this experiment do not seem great enough to account for such a large difference. The method of computation used by Adler and Fliegelman is not made clear in their article. Using our methods of computation on their data (0.9 mm. displacement of the trace at a distance of 1500 mm.) one obtains a value approximately half as large as that which they have computed from the same data (1.034 min. instead of 2 min. 14 sec.). This value of 1.034 min. falls within the range of the extents of the small rapid motions recorded in the present experiment. In general, there appears to be good qualitative agreement between Adler and Fliegelman's results and those obtained in this experiment. Quantitatively, except with respect to frequency, there is rather poor agreement —especially in the case of the small rapid motions.

Lord and Wright state that in their records there is little evidence of motions having frequencies of the order of 50 cycles per sec. However, if the extents of these motions are generally less than one min. of arc, as the present experiment indicates, such motions should not be discernible in their records, because they state that their recording apparatus was not sensitive to movements smaller than one min. of arc. Their negative results concerning these small movements indicate that if they do exist they must be smaller than one min. of arc—a conclusion which agrees with the results of the present experiment.

The method used in this experiment provides a more efficient optical arrangement than some of the methods used by other investigators. The major criticism of this method is that the recording apparatus requires the use of an attachment to the eye. As Hartridge and Thomson have pointed out (5) there is a possibility that such an attachment will not follow the motions of the eye accurately.

There appears to be no simple method of checking directly on this problem of lens slippage. However, there is considerable indirect evidence to support the assumption that the lens follows the small movements accurately. First, a contact lens which is well fitted to the eye adheres with great force because of surface tension. Removing the lens is not easy until this tension is broken. (One subject, LR, was fitted with a very

tightly adhering contact lens which was extremely difficult to remove. The records obtained by means of this lens are essentially the same as those obtained from the same subject while wearing a relatively loose-fitting lens.)

Second, one might expect the contact lens to vibrate with a frequency corresponding to its natural period if it does not follow the actual movements of the eye. However, the records obtained with the very small mirror placed directly on the eye are quite similar to those obtained by means of the contact lens. Certainly the natural periods of these two very different attachments to the eye are not the same. The mass of the small mirror placed directly on the eye was only a very small fraction of the mass of the contact lens and mirror. In the case of the contact lens one might expect the friction of the lids to offer some slight resistance to the movement of the lens, yet the records obtained by means of the small mirror which did not touch the lids indicate that this was not an important factor. It seems very unlikely that these two very different attachments to the eye would yield such similar results unless each were following the movements of the eye with a fair degree of accuracy.

Summary

1. The purpose of this experiment was to measure rapid involuntary movements of the eye (physiological nystagmus) during steady monocular fixation of a stationary object.

2. A photographic method of recording was used. An optical lever was provided by the use of a mirror mounted in a contact lens worn by the subject. Any movement of the eye of the subject resulted in a proportional displacement of the image of a light source on moving recording paper. Prisms properly placed in the optical system made possible the simultaneous recording of vertical and horizontal components of the motions of the eye and of the head. A fixation point at an optically infinite distance eliminated many problems associated with involuntary head movements. Records were obtained from five subjects.

3. Four main 'types' of movement are to be seen in the records. (a) Small rapid motions with a median extent (peak to trough) of about 17.5 sec. angle of rotation, and frequencies ranging from 30 to 70 cycles per sec. (b) Slow motions of irregular frequency and extent. (c) Slow drifts in one direction or another upon which the motions mentioned above are superimposed. (d) Rapid jerks with an average extent of about 5.6 min. of arc occurring at irregular intervals, at times apparently compensating for the drifts. Total movement due to the combined effects of these motions over a period of three or four sec. was usually less than 10 min. of arc during the steadiest fixations.

4. On the basis of the above results conclusions were drawn regarding the motions of the retinal image of the object being fixated. (a) During a fixation period of three to four sec. the image of the object being fixated may move across a total of 25 to 50 receptors. (b) The slower motions, drifts, and jerks may carry the retinal image across about a dozen receptors. (c) The small rapid motions move the retinal image across two or three receptors at most. However, the median extent of these motions is so small that the movement of the retinal image under optimal conditions of fixation is not likely to exceed the width of one receptor.

5. The results of this experiment indicate that small rapid motions of the eye do occur during 'steady' fixation, but that the magnitudes of these motions are extremely small.

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THE GENERALIZATION OF AN INSTRUMENTAL RESPONSE TO STIMULI VARYING IN THE SIZE DIMENSION

BY G. ROBERT GRICE AND ELI SALTZ

University of Illinois

One of the most important concepts in modern learning theory is that of stimulus generalization. Hull has made extensive use of the concept in his behavior theory (9, 10), and it is the concept by means of which Spence (19, 20, 21) was able to predict several of the transposition phenomena in visual discrimination learning. The importance of generalization is widely accepted. Yet in spite of the theoretical significance of the problem, relatively few experimental studies have been directly concerned with it. While the existence of generalization gradients has been adequately demonstrated (1, 2, 3, 4, 7, 8, 11, 13, 16, 17, 18), there is relatively little evidence concerning the precise nature of the functional relationships between the amount of generalization and various stimulus dimensions.

The most frequently cited studies are those of Hovland (7, 8), employing auditory stimuli and the conditioned galvanic skin response. Hovland found generalized response strength to be a negatively accelerated decreasing function of the number of j.n.d.'s difference in pitch or loudness between the test stimulus and the training stimulus. The gradient for intensity was much flatter than the pitch gradient. Humphreys (13), using the same response, and frequency of tones as the stimulus dimension, also found the negatively accelerated gradient, but found a very flat or possibly a positively accelerated gradient if conditioning occurred under conditions of partial reinforcement. Until recently, there was very little data involving the generalization of instrumental responses. Brown (3) demonstrated generalization of a running response in the rat to three degrees of brightness. Schlosberg and Solomon (18), studying latency in a jumping stand choice situation, report data from four rats in which log latency was a linear function of difference in brightness from the initial positive stimulus. Their brightness scale was an equal appearing intervals scale based on human judgments. Recently, studies by Frick (4) and Raben (17) have demonstrated generalization of instrumental responses to different levels of illumination, but yield no evidence as to the form of the primary gradient, since both studies involve discrimination training from the beginning of practice. Differential reinforcement, i.e., reinforcement of the positive stimulus and non-reinforcement of the negative, results in the narrowing of the range of empirical generalization gradients. Thus the form of such response gradients becomes a function of the number of non-reinforced trials to the generalized stimuli. This was especially well demonstrated by Raben. However, gradients obtained in this way do not reflect the primary generalization of habit strength in any simple way, but may best be interpreted as a joint function of generalized habit strength and inhibitory factors. In order to obtain a function which may be regarded as indicating the nature of primary stimulus generalization, it is necessary to build up habit strength to one stimulus and *then* to test for generalization to other stimuli on the dimension. For this purpose, no extinction of responses to the test stimuli should be introduced prior to the generalization test.

On the theoretical level, Hull (10) has assumed, on the basis of Hovland's data, that the generalization gradient of habit strength is symmetrical and negatively accelerated when plotted on a j.n.d. scale. With respect to intensity dimensions, he has recently predicted (12) that response gradients to stimuli of higher intensity will be convex, while those to stimuli of

less intensity will be concave. (This is predicted because response strength is a function not only of habit strength, but of stimulus intensity *per se*.) Spence (21), in his transposition theory, has assumed that the generalization gradients for the visual size dimension are convex when plotted on a logarithmic scale of area. The only evidence to support this assumption is the success of the theory in predicting transposition phenomena. No studies directly concerned with the generalization of instrumental responses to stimuli varying in the size dimension have been performed. The obtaining of such data seems desirable, both in relation to the problem of generalization as a whole, and in relation to the more specific discrimination learning theory. If empirical functions could be obtained for both generalization of habit strength and inhibition, rather precise predictions of transposition and related phenomena should be possible if the general principles of the theory are valid.

The present study is an attempt to determine the form of the stimulus generalization gradient for the visual size dimension in the white rat. Animals were trained to make a simple instrumental response to white circles of a particular size, and then separate groups were tested for amount of generalization to circles of differing sizes.

Experimental Procedure

Subjects.—The subjects were 135 experimentally naive albino rats from the colony maintained by the Department of Psychology at the University of Illinois. There were 70 males and 65 females, the sexes being approximately evenly distributed among the experimental groups. The ages of the subjects ranged from 70 to 110 days at the beginning of the experiment. The animals' diet during the experiment consisted of seven grams of Purina Laboratory Chow administered in individual feeding cages after each daily experimental session. They were adapted to this schedule for a period of one week prior to the beginning of the experiment.

Apparatus.—The size stimuli employed in this experiment were white circles which have been previously described (5). Briefly, they were white disks cut from sheet metal, with a small square door in the center. The door, which was flush with the surface of the disk, could be pushed open by the rat's nose. Attached to the back of the disk, just below the door, was the food dish, and the rat could easily obtain the food by pushing its nose through the door. The incentive used was pellets made of a mixture of Purina Chow and flour, which averaged 0.15 gm. in weight. The circles used in the experiment were 79, 63, 50, 32, and 20 sq. cm. in area. These represent equal logarithmic steps in area with the exception of 63, which is a half step between 79 and 50.

The apparatus in which the stimuli were presented has also been previously described (6). In brief, they were presented at the end of a 24-in. runway. The runway contained vertically sliding doors at each end and in the middle. It was mounted on a pivot at the center so that each half could become in turn the starting end and the reaction end. By rotating the alley after each trial, it was unnecessary to handle the animal except at the beginning and end of the experimental session. Fig. 1 is a schematic floor plan of the apparatus. The circle was housed at the front of a two-inch continuation of the alley which could be slid back to permit rotation of

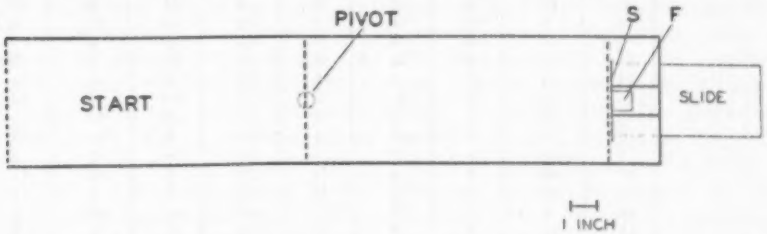

Fig. 1. Floor plan of the apparatus. Heavy dotted lines indicate vertical sliding doors. The dotted circle indicates the point at which the 24-in. alley was pivoted. S indicates the stimulus circle, and F the food dish. The two-in. section of alley containing the circle could be moved back on the slide block to permit turning the long alley.

the alley. All of the circles were mounted with their centers equally distant from the floor. Thus, irrespective of the size of the circle, the small door was the same distance above the floor and the response to be made to each circle was the same.

Preliminary training.—The first step was to train the animals to open the small door in the circle and obtain food. On the first day the animal was placed in the reaction compartment with the door in the circle open, and was allowed to eat 10 pellets from the food dish. Then the alley was rotated and the rat ate 10 more pellets with the other end of the alley as the reaction compartment.

On the second day, the rat was placed in the starting compartment and was allowed to enter the reaction compartment and eat one pellet with the stimulus door wide open. Then the alley was rotated and the second trial was run with the door closed slightly. In all, 10 such trials were run with the door being more nearly closed on each trial. By the ninth and tenth trials the door was completely closed. Five animals which did not learn to open the door with this training procedure were eliminated.

The circle used in preliminary training was the one to be used in further reinforcement training. Animals to be tested for generalization to larger stimuli were trained on the 20 sq. cm. circle, and those to be tested for generalization to smaller stimuli were trained on the 79 sq. cm. circle.

Reinforcement training.—On each of the next three days the rats were given 20 trials of opening the door to obtain food. The alley was rotated between trials, and the trials were run in immediate succession—allowing only time for the rat to eat and for a new pellet to be placed in the food dish. Latencies were recorded from the time the door from the starting compartment was opened until the rat's nose moved the door in the stimulus circle. As soon as the food was obtained, the vertical sliding door in front of the stimulus circle was lowered, and the animal was allowed to eat the food before rotating the alley for the next trial.

The experimental work was conducted between the hours of one and five in the afternoon. Each animal was run at the same time every day.

Generalization tests.—The tests of generalization consisted of a series of 25 extinction trials to the stimulus being tested. Animals being tested for generalization to larger stimuli were trained on the 20 sq. cm. circle and groups of 15 subjects each were then tested on circles of 20, 32, 50, and 79 sq. cm. Animals being tested for generalization to smaller stimuli were trained on the 79 sq. cm. circle and groups of 15 were extinguished on circles of 79, 63, 50, 32, and 20 sq. cm. The 63 sq. cm. circle (one-half of a log step between 79 and 50) was added after an inspection of preliminary data indicated the desirability of further information concerning the function in that area.

On the extinction day, the animals were first given five additional reinforced trials to the original stimulus. Then extinction trials to the test stimulus were begun without a break and were

TABLE I

NUMBER OF RESPONSES IN 25 EXTINCTION TRIALS

Each value in the body of the table is the result of a single animal

Training Stimulus	79 Sq. Cm.					20 Sq. Cm.			
Test Stimulus	79	63	50	32	20	20	32	50	79
	15	16	5	9	7	9	22	8	12
	18	8	10	8	7	19	13	11	4
	9	12	10	8	0	10	17	2	1
	11	5	7	14	8	21	20	3	8
	13	9	17	5	11	10	8	4	14
	20	10	17	11	7	18	22	6	14
	9	12	11	9	9	11	12	10	5
	13	8	7	16	9	18	8	13	8
	5	11	7	7	2	23	14	15	4
	10	18	6	4	6	10	9	10	11
	22	12	6	9	8	10	8	4	3
	18	8	5	8	3	10	14	10	5
	17	11	7	8	1	9	13	4	5
	10	10	4	8	0	10	16	8	4
	12	12	9	10	0	8	4	1	0
Mean	13.5	10.8	8.5	8.9	5.2	13.1	13.3	7.3	6.5

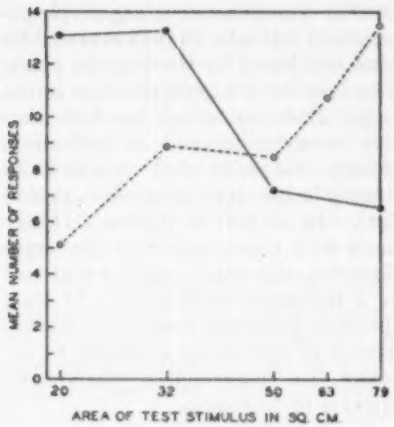

FIG. 2. Generalization functions showing mean number of extinction responses for each test group

run one immediately following the other. On these trials, the door in the circle was locked so that it would open only ½ in. and there was no food in the food dish. All of the circles were identical except for size, and were mounted so that their centers were all the same distance from the floor. For animals tested on a circle of the same size as that during initial training, a new circle of the same size was substituted. The criterion of a response was sufficient contact of the animal's nose with the door in the circle to move it. Latencies were recorded, and if the animal failed to respond in 60 sec., the trial was scored as a failure of response. The measure of generalization employed was the number of responses made during the series of 25 extinction trials.

RESULTS

The number of responses made by each animal during the extinction series and the mean for each group are given in Table I. The generalization functions obtained are presented graphically in Fig. 2. Mean number of responses during the test series is plotted as a function of area of the test stimuli in logarithmic units. The solid line represents generalization to stimuli of larger size and the broken line represents generalization to smaller stimuli. It may be seen that responses were made to all stimuli tested. In other words, generalization did occur; it extended in both directions; and all of the stimuli used fell within its range. Both gradients show a decreasing amount of generalization as a function of the difference in size from the training stimuli. This decrease was statistically significant. Analyses of variance were applied to each gradient to test the hypothesis that the means were equal. The *F*-values were 8.61 for generalization to larger stimuli and 9.59 for generalization to smaller stimuli, which, for the appropriate degrees of freedom, were both significant at the .001 level of confidence. Thus the null hypothesis may confidently be rejected in both cases.

With respect to the forms of the functions, it is first apparent that the two gradients are different. The gradient for generalization to smaller areas is concave or negatively accelerated, while the gradient for generalization to larger stimuli is first convex or positively accelerated and then becomes negatively accelerated. An analysis was performed to test the hypothesis that the two functions are of the same form. For this purpose one function was reversed, both being plotted as functions merely of the number of log steps between the training and test stimuli. (The 63 sq. cm. point was omitted for this purpose since there was no corresponding point on the other curve.) The test employed was Case 9 suggested by Lindquist (15). Specifically the hypothesis tested was that the means of successive differences between the points on the two curves was equal to zero. The analysis yielded an *F* of 6.73 which was significant at the five percent level of confidence, and approached significance at the one percent level. (F = 6.87 required for significance at the

one percent level.) In other words, the test indicated the probability that the populations from which the two curves were drawn do not coincide and that the apparent difference in the functions is probably genuine.

Discussion

Probably the first point which should be mentioned in connection with the present data is that again the existence of generalization gradients has been experimentally verified. This comes as no great surprise, but is of interest in relation to the contention of Lashley and Wade (14) that such gradients, when obtained, are experimental artifacts due to training and testing under conditions which involve poor opportunity for discrimination. They argued that when stimuli are presented successively, allowing no opportunity for direct comparison, such gradients result merely from failure of the animal to discriminate. The present experiment was conducted in a situation where it has been experimentally demonstrated that discrimination learning proceeds equally well whether there is opportunity for direct comparison or not (6). Consequently, the Lashley and Wade explanation is invalid for the present experiment, and cannot legitimately be raised as an argument against the theory which assumes a spread of habit strength to other stimuli in the dimension.

The fact that the two functions obtained are different in form is contrary to the assumptions of Spence (19) and Hull (10) who have assumed the functions to be symmetrical. (It will be pointed out later, however, how Hull's assumption does not necessarily lead to the prediction of symmetrical *response* gradients for intensity dimensions.) The function for generalization to smaller stimuli which is negatively accelerated is similar to that assumed by Hull and found by Hovland for pitch. The function for generalization in the larger direction, which involves positive acceleration and an inflection, closely resembles that assumed by Spence in his most recent formulation (21). In so far as Spence's theory deals with transposition in the larger direction, this result may be regarded as a tentative verification. At least the basic principle of assuming the existence of decreasing gradients to account for transposition phenomena appears to be sound.

A point of further interest in connection with the present study is its possible relation to Hull's recent discussion (12) of the 'stimulus intensity dynamism.' In this discussion Hull assumes that the primary generalization gradient for reaction potential ($_sE_R$) is of the simple negatively accelerated type. He further assumes that reaction potential is also an increasing function of stimulus intensity. *Empirical* generalization functions are then assumed to be a multiplicative function of generalized reaction potential and the stimulus intensity factor. This theorizing leads Hull to five predictions regarding the nature of generalization functions for intensity dimensions. A discussion of the present experiment in this connection would, of course, be contingent on the assumption that the visual size dimension may properly be regarded as an intensity dimension. Since white stimuli were employed, the total amount of reflected light would be a function of stimulus area. This suggests that at least a tentative examination of the data with respect to Hull's predictions would be worthwhile.

The design employed here is the type of design to which Hull's predic-

tions apply. A particular part of the dimension was selected, and generalization to larger sizes was studied from the lower extreme, and generalization to smaller sizes was studied from the upper extreme. Thus, both gradients fall within the same range of stimulus values. Hull's predictions together with the present findings concerning them are as follows:

1. The effective gradient for generalization toward more intense stimuli will be convex. This prediction is partially fulfilled since the obtained gradient does show convexity before its inflection. The inflection, however, was not predicted.

2. The effective gradient for generalization toward weaker stimuli will be concave. This prediction was fulfilled.

3. "The origin of the gradient originating at the stronger stimulus extreme will have a stronger reaction potential than that originating at the weaker stimulus extreme." The obtained means were 13.5 and 13.1 with the difference in the direction predicted, but the difference is definitely insignificant. This prediction cannot be regarded as clearly substantiated.

4. The gradient for generalization to weaker stimuli will show a greater drop than that for generalization to stronger stimuli. The drop between the point of origin and the extreme test point was slightly greater for the generalization to smaller sizes, but an analysis of variance revealed that this difference was not statistically significant.

5. "The two gradients intersect at their mid-point." This prediction was essentially verified.

In general, the correspondence between the obtained gradients and Hull's predictions is quite striking. This is especially true of the predicted asymmetry of the two curves, which was essentially verified except for the inflection in one curve. In this connection it is perhaps of interest to note that the experiment was designed and completed prior to the publication of Hull's paper, and that Hull was unaware of the present data when the theory was formulated. The reasons for the failure to verify all of the predictions is not at once apparent. The possibility that the discrepancies may be due to sampling error cannot be rejected. There is also the question as to whether size may be properly regarded as an intensity dimension. And, of course, future data may reveal the need for further refinements or modifications of the theory.

Summary

1. One group of 60 white rats was trained to open a door in the center of a white circle 20 sq. cm. in area. Subgroups of 15 were then tested by means of 25 extinction trials on circles of 20, 32, 50, and 79 sq. cm. A second group of 75 rats were trained to respond to the 79 sq. cm. circle and sub-groups were tested on circles of 79, 63, 50, 32, and 20 sq. cm.

2. Functions were obtained showing generalization to stimuli both larger and smaller than the training stimulus. Both gradients showed a significant decrease in response strength as a function of increasing difference in size from the training stimulus. Response strength to smaller stimuli was a negatively accelerated decreasing function of log area. The function relating log area and response strength to larger stimuli was inflected, showing initial positive acceleration.

3. The results are interpreted as supporting the validity of the concept of stimulus generalization as employed by such theorists as Hull and Spence.

They lend further support to the use of the concept in the interpretation of visual size discrimination learning phenomena.

4. The data are further interpreted as being in essential but not complete agreement with recent predictions made by Hull on the basis of a stimulus intensity factor.

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THE EFFECT UPON LEVEL OF CONSUMMATORY RESPONSE OF THE ADDITION OF ANXIETY TO A MOTIVATIONAL COMPLEX[1]

BY ABRAM AMSEL

Newcomb College, Tulane University

INTRODUCTION

Experimental studies (2, 7, 8) have seemed, thus far, to indicate that the addition of emotionally-derived needs to previously established and controlled motivational complexes may either increase or decrease the strength of responses which have been present in particular situations.

If, as Hull (4) postulates, (1) the addition of any need state to an existing motivational complex increases the strength of the total generalized motivational condition, and, (2) the strength of any ongoing response is directly related to the strength of this total drive strength, then it follows that the addition to a motivational complex of any need state must increase the strength of responses isolated for observation in a controlled situation. This, according to the position taken by Amsel and Maltzman (1), will be the case provided the additional need state does not carry with it a drive stimulus which has become connected, in the situation in question, to responses which compete with the criterion response. Amsel and Maltzman designed their experiment as a partial test of this position. It can be summarized as follows:

Twenty white rats were trained to drink for 10 min. each day in individual drinking cages of identical design. When the *S*s, as a group, had reached a relatively stable limit of drinking in the 10-min. period, the procedure was modified so that each *S* was given electric shock through a grid floor for 20 sec. immediately before the drinking period. The shock was administered in a situation *different from that in which the Ss had learned to drink*. It was assumed that at the time of drinking, after shock, a state of emotionality existed in the *S*s. During the shock-before-drinking period, the drinking curve showed a significant increase over the previously established limit. It was reasoned that the increase in drinking occurred because the emotionality, as an added component to the motivational complex, was not conditioned, *in the drinking situation*, to responses which could compete with the drinking behavior. The introduction of emotionality was thought of as simply raising the general motivational level and, hence, as increasing the level of drinking behavior.

The present experiment was designed as a further test of the position stated above. It sets forth conditions in which the added emotionally-derived need provides a drive stimulus which is conditioned to responses which may compete with the previously established drinking pattern. Our purpose is to show that a decrease in the strength of consummatory response, upon the addition of an emotionally-derived need state to a previously existing motivational complex, may be due to the presence of competing response tendencies conditioned to the added drive stimulus.

The experimental plan is to condition the secondary drive of anxiety (fear) to stimuli previously connected with drinking behavior and, subsequently, to measure the amount of drinking in the presence of those stimuli. The arousal of anxiety in the drinking situation should not only increase the total generalized drive strength, but should also provide anxi-

[1] This research was supported by a grant from the Tulane University Council on Research.

ety drive stimuli which are conditioned to anxiety-reducing responses. Insofar as these responses are incompatible with the previously established drinking behavior, there should result a diminution in the strength of the drinking response as measured by the volume of water intake.

Experimental Method

Subjects and Apparatus

The subjects for this experiment were 25 female albino rats from a colony maintained by the Department of Physiology of Tulane University. All of the *S*s were of the same strain and were between three and four months old at the start of the experiment.

The apparatus employed was a rectangular box with a grid floor and glass top, which could be divided into two compartments by means of a guillotine door which ran the width of the apparatus. The entire apparatus was 20 in. long, nine in. wide, and seven in. high in its inside dimensions. The guillotine door divided it into two sections, a smaller one which was 6 × 8 in., and a larger one which was 14 × 8 in. Henceforth, the smaller section will be referred to as section Y, the larger one as section X. The walls of the apparatus were painted white, and white paper was kept under the grid floor at all times.

The grid floor was constructed of brass rod of $\frac{1}{16}$ in. diameter spaced $\frac{1}{2}$ in. apart. Alternate grid bars were wired and connected to the terminals of an instrument which produced the shock employed, when necessary.[1] This was a constant current D.C. supply, which was preset through a calibrating resistance of 300,000 ohms —the estimated average resistance of the laboratory rat (9)—and which delivered short pulses of shock, the duration and rate of which were electronically controlled.

In the wall at one end of the apparatus, in section Y, a small opening was made so as to permit the insertion of the orifice of a graduated drinking device. This device allowed the measurement of water consumption to the nearest $\frac{1}{10}$ ml.

Experimental Procedure

After the *S*s had reached a stage of adjustment to handling necessary for experimentation, they were put on the following drive schedule: 4 P.M. —Water removed from cages; 8 A.M.—Each *S* given four gm. dry food in individual cage; 8:30 A.M.—Water intake measured (or other procedure introduced); 10 A.M.—*S*s replaced in individual cages with water and eight gm. food. Hence, the thirst drive in the present experiment was defined by 16½ hrs. water deprivation and the ingestion of four gm. of dry Purina Dog Chow immediately before the water intake was measured. The drive regimen outlined above was maintained throughout the experiment.

The experimental procedure itself can be divided into three parts. In their chronological order they are: (1) preliminary measurement of water intake, (2) introduction of the experimental variable, and (3) test measurement of water intake. Two groups of *S*s were differentiated, to be called Group A (anxiety), and Group C (control).[2] They are differentiated according to a difference in the second part of the experimental procedure.

Preliminary (control) measurement of water intake

This part of the procedure was identical for Groups A and C. During this period, water intake was measured for each *S* following 16½ hrs. water deprivation and the ingestion of four gm. of dry food. The exact procedure was as follows: (1) *S* placed in section X of apparatus, (2) 10 sec. allowed to elapse, (3) the guillotine door separating X from section Y was raised and kept open while (4) *S* was permitted to remain in the apparatus for five min. with the water bottle inserted and water always available, (5) *S* was removed to a small cage and a measurement was made of the amount of water consumed. One such trial was carried out with each *S* during every day of this period. This procedure was continued with both groups until an apparent maximum levelling off had been achieved in the mean drinking curves. For both groups this period was of 24 days duration.

Introduction of the experimental variable

This part of the experimental procedure covered a period of four days. During these four days, each *S* was given one trial per day of 25 sec. duration. Section Y of the apparatus was never employed during this period, and water was never available during this time. For the 13 *S*s of Group A, the following procedure was followed: (1) *S* was placed in section X of the ap-

[1] The shocking instrument was designed and built by Mr. Richard G. Davis of the Department of Physics, Tulane University.

[2] Although Group C was run primarily as a control for certain possible variables in the performance of Group A, the two groups were not run at the same time. Group A was run during April and May by Miss Beverly McArthur and Group C was run during June and July by Mr. Keith Cole.

paratus, (2) after five sec. had elapsed a shock of intensity 0.1 milliampere was applied through the grid floor for five sec., (3) the shock was turned off and remained off for five sec., and was then reapplied for five sec. more, (4) following the last five sec. of shock *S* remained in section X for five sec. and was then removed to a small cage. This procedure with group A was designed to condition an anxiety (fear) response to the various cues in the apparatus (grid, white walls, etc.).

During this experimental period, the 12 *S*s of Group C were differentiated from Group A in that they were not shocked. They were merely placed in section X of the apparatus and allowed to remain for 25 sec. following which time they were removed to their cages. The purpose of this procedure was to control for any effect upon group A of the change in procedure itself, regardless of the presence of shock and to insure that any effect produced in group A could be attributed to shock.

Test measurement of water intake

This period in the experimental procedure followed immediately the four-day experimental period and served as a test of the effect upon water intake of the experimental manipulation. Each day of the test period was identical with those of the preliminary period previously described, i.e., water intake was measured in the manner indicated. In addition, a count was made of the number of animal droppings during each day of the test period. Through an unfortunate oversight, this count was not made on the first two days of the test period, but started on day three.

During the time Group A was being run, the temperature in the room in which the animals were housed varied between 74 and 78 degrees. The temperature range, during the running of Group C, was 80 to 84 degrees.

Results and Discussion

Figs. 1 and 2 summarize graphically the data; those from the Group A condition are shown in Fig. 1, those from the Group C condition are shown in Fig. 2. Since a definite trend in the mean drinking curve developed with Group A, 21 trials were run in the test series; only five trials of test drinking were run with the Group C subjects,

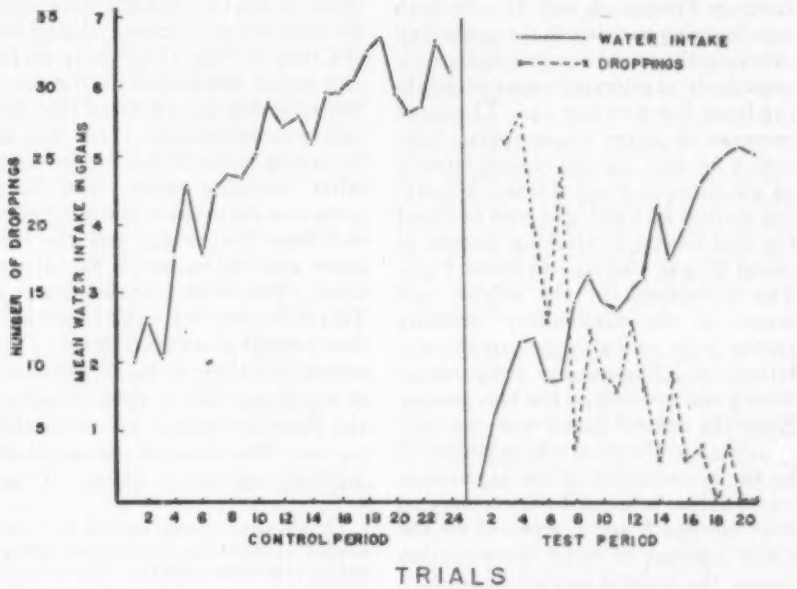

Fig. 1. Curves showing mean water intake during preliminary control period, and relationship of number of fecal droppings to mean water intake during test period for Group A

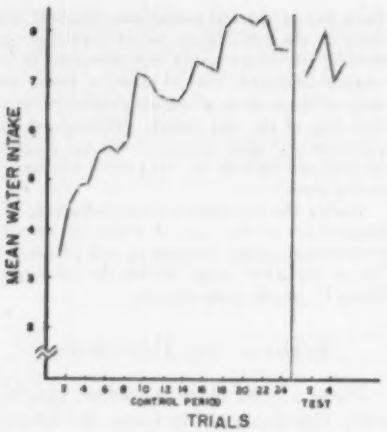

FIG. 2. Curves showing mean water intake during the preliminary control period, and during the five-day test period for Group C

since no consistent change in mean drinking seemed to be developing.

The 24 days of preliminary water intake measurement yielded similar pictures in Groups A and C. In each case learning to drink in the controlled drinking situation was evidenced by a negatively accelerated curve of drinking from day 1 to day 24. The mean increase in water consumption from day 1 to day 24 was approximately 4½ gm. in each group: Group A drinking started at 2 gm. and rose to about 6½, and Group C drinking started at about 3½ gm. and rose to about 8 gm. The differences in the origins and limits of the preliminary drinking curves were probably due to the difference in living-quarter temperature during the running of the two groups. Since the control group was run only as a check on the gross effects produced by the introduction of the experimental variable, it seemed unnecessary to have Groups A and C equated for absolute amount of water consumption during the control drinking period.

The important feature of the results was the effect upon water consumption of the introduction of anxiety into the drinking situation. This effect can be appreciated by comparing, in Fig. 1, the level of water intake on the last days of the control period (before the introduction of anxiety) with the level of water intake on day 1 of the test period (after the introduction of anxiety into the drinking situation). The mean water consumption on day 24 of the control period was 6.17 gm., that on day 1 of the test period was 0.31 gm. Of the 13 *S*s of Group A, 12 drank zero gm. on the first test day after the conditioning of anxiety to the drinking situation. The other *S* decreased its water consumption from 7.4 to 4.0 gm.[4] This should be compared with the drinking performance of the control group. Of the 12 *S*s in this group, seven drank less after the four-day, no-drinking, period than on the last day before it, and five *S*s drank more after than before. Since there seemed to be some reduction in the level of the average drinking curve of Group C (Fig. 2) following the four-day period designated by the vertical line after day 24, a *t*-test of this difference was performed. This was done by taking, for each *S*, a 'before' and an 'after' drinking score. The 'before' score was the mean of the last five control days (20 to 24) and the 'after' score was the mean of the five test days. The *t*-value obtained was 1.5. This falls short of significance at the five percent confidence level. There seems, therefore, to be no pronounced or significant effect upon drinking of the four-day period of no drinking, *per se*. The pronounced decrease in drinking shown in Group A may,

[4] It is important to note that the *S*s were never shocked while drinking or while approaching the orifice of the water container. The reduction in drinking cannot, therefore, be due to avoidance of the drinking locus, as in the case of Masserman's cats (6).

therefore, be attributed to the presence, during the no-shock test period, of anxiety, built up during the four-day experimental period through shock and shock reduction.

Fig. 1 shows, also, a curve of recovery of drinking in Group A, as a function of successive test period trials. As in the preliminary drinking period, the water intake increased with successive five-min. drinking sessions. The curve shows a general negative acceleration from day 1 to day 21 of the test period. It appears to be levelling off at an average water consumption of about five gm., somewhat lower than the limit of mean water intake reached during the preliminary drinking period.

Corresponding to the curve of recovery of drinking during the test trials in Fig. 1, is a curve showing the number of fecal droppings from day 3 up to and including day 21 of the test period. Each point on this curve represents the total number of droppings of 13 *S*s on the particular test trial. A curve fitted to these data could be described by a negatively accelerated decreasing function. This curve is, then, approximately the inverse of the water intake curve to which it corresponds.

Since defecation was never observed in Group C during the test trials, nor in either group during the latter preliminary trials, in the drinking situation, it may be considered as a type of anxiety-motivated response. Other studies (3, 5, 10) have also considered that excessive defecation is a sign of emotional disequilibrium in rats.[5] It seems reasonable, therefore, to consider the inverse relationship of number of droppings to the ordinal number of the test trial following the four-day shock period as an indication that the rise in the test period drinking curve is related to the gradual diminution of anxiety in the drinking situation. This would imply that the tendency to drink had always been present during the test trials but that immediately after the experimental shock period, the anxiety-motivated responses had successfully competed with the drinking behavior.

The results of the present experiment with their interpretation lend support to the following set of statements: (1) When anxiety is conditioned to a situation which has, in the past, given rise to consummatory behavior, the introduction of anxiety will reduce the level of such consummatory behavior. (2) The reduction in consummatory behavior is due to response competition, and not to any reduction in the generalized motivational state resulting from some interaction of needs. (3) Successive exposures to the situation which has been conditioned to both the consummatory behavior and to anxiety will, in the absence of further reinforcement of anxiety through pain reduction, bring about a reduction in the strength of the anxiety-motivated behavior, and, consequently, a recovery of the consummatory behavior.[6]

[5] Humphrey and Marcuse (5) have published a plot of number of defecations against 25 successive days of handling of untamed rats which is similar to the present curve showing number of droppings on successive test trials.

[6] A study by Wedemeyer (11) has shown that anxiety-motivated behavior may persist even if anxiety is not reinforced by shock and shock-reduction, so long as the exposures to the anxiety-evoking situation are short and well distributed. Massing of trials was shown to bring about a reduction in strength of the anxiety-motivated response. The five-min. exposures of the present experiment are probably equivalent to daily massed-trial sessions in which, using Hull's terminology (4), work inhibition is built up providing the basis for the growth of conditioned inhibition of the anxiety reaction to the drinking-situation stimuli.

Siegel and Siegel (10) have concluded, on the basis of experimental evidence, that emotionality induces a state of thirst in the rat. They state, supported by the writings of Cannon, that, ". . . a state of inhibited salivary flow attends the extreme emotions of anxiety of fright." If the presence of anxiety, during the test trials, did increase the intensity of the need for water in the present experiment, such an effect could not be inferred from the observed behavior. It remains possible, however, that this effect was obscured by the presence of the competing, anxiety-reducing tendencies. In any event, it is apparent that prediction of the results of the present experiment purely on the basis of the physiological relationships put forth in the Siegel and Siegel study would have been in error.

It seems reasonable, on the basis of the present results and those of Amsel and Maltzman, to maintain the position that, (1) the addition of any emotionally-derived need to a motivational complex increases the total motivational strength of the organism at that time; and that (2) observed decreases in behavior strength, upon the addition of an emotionally-derived need to a motivational complex, are due to the introduction, with the new need, of a drive stimulus which has been connected with responses which are incompatible with the criterion response.

The above position would gain strength if the following experimental result were obtained: that subjects trained to respond (e.g., drink) in a specific situation show an increase in the strength of such response when anxious, where the anxiety has been aroused immediately before introduction to the drinking situation by cues other than those associated with the drinking behavior.

Summary

The following hypothesis was tested: that the addition of anxiety, a secondary emotionally-derived need, to an existing motivational complex would reduce the strength of a consummatory (drinking) response.

The subjects were 25 female albino rats. They were trained to drink for five min. each day, following a fixed deprivation period, in a specific situation. When a limit of mean water consumption was reached, under such conditions, 13 of the *Ss* (Group A) were shocked in the drinking situation over a four-day period; the other 12 *Ss* (Group C) were not shocked during the four-day interval, but were treated like Group A in all other respects. Water was never available during the four-day experimental period. After the experimental interval, both groups were tested for water intake, as before, with no shock ever present.

The data indicated that the Group A curve of water intake fell to almost zero at the beginning of the test period; the Group C curve did not change significantly following the no-drinking interval. Associated with the decrease in water intake of Group A was a marked onset of frequent defecation in the drinking situation by these *Ss*. A subsequent rise in the drinking curve over a period of 21 days, in Group A, was associated with a parallel decrease in the number of fecal droppings.

The results were interpreted as indicating, or, at least, suggesting that the diminution in water intake of Group A was due to the presence of anxiety-reducing responses in the drinking situation, which competed with the previously-established consummatory response.

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EYELID CONDITIONING AS A FUNCTION OF THE INTER-TRIAL INTERVAL

BY KENNETH W. SPENCE AND EUGENIA B. NORRIS[1]

From the Psychological Laboratory, University of Iowa

Introduction

The effect of spacing practice trials on learning has been found, among other things, to be a function of the type of problem involved. While in most situations learning is speeded up when the trials are spaced rather than massed, instances have occurred in which this variable has had little or no influence (6). Moreover, on the basis of the type of theory of inhibition proposed by Hull (4), it is possible to specify learning situations in which the massing of trials should speed up mastery of the problem. Thus, corollary XVI in Hull's *Principles of behavior*, which is derived from his assumptions concerning reactive inhibition (I_R), states that "the rate of acquisition of the preference for the behavior sequences involving the less energy expenditure or work will be faster with massed than with distributed reinforcements" (4, p. 295).

Similarly, it may be predicted from such a theory as Hull's that massing will speed up the learning or at least the first portion of the acquisition period of simple trial and error problems in which the correct response is relatively low in the response hierarchy and the initial portion of the learning consists to a considerable extent in the weakening (or extinguishing) of the initially stronger, incorrect responses. Support for one of the basic assumptions underlying the latter prediction is to be found in the recent evidence showing that massing of trials speeds up the extinction of conditioned responses, both classical and instrumental (8, 10).

The effect of varying the inter-trial interval on any learning situation will depend, then, upon the responses elicited in the situation, i.e., whether these responses are the correct ones or the wrong ones. Increase in the inter-trial interval permits whatever responses have been occurring more time to recover from the decremental effects of the work expended in their occurrence. If the responses occurring have been predominantly wrong ones that need to be eliminated, spacing of trials will slow up the learning. If the response occurring is the correct one, spacing will speed up the acquisition, and massing will slow it down.

Most of the studies of this experimental variable have involved relatively complex learning situations, such as rote and motor learning with human subjects and serial maze learning with animals. There have been no very extensive systematic investigations of this variable in either simple classical or instrumental conditioning situations in which the elimination of competing responses is at a minimum. Such studies are sorely needed, not only for the reason that they provide a more ideal situation in which to test such hypotheses as Hull's but also because we need to discover the laws relating this variable to acquisition in such relatively simple response situations.

The only investigation that has used more than two different inter-trial intervals is that of Calvin with eyelid conditioning in human subjects (2). Hull (4) reports that Calvin, giving the trials at the rate of 3, 9 and 18 trials per min., obtained superior conditioning with the three per min. rate than the other two

[1] Now at the University of Rochester.

rates which were about equal in effectiveness. Humphreys (5) found that an inter-trial interval of 60 sec. produced significantly better eyelid conditioning in humans than one of 30 sec. Finally, Reynolds, using intervals of 15 and 90 sec. obtained superior conditioning in three of four groups paired for another variable (S_c–S_u interval) with the longer inter-trial interval. There is a definite suggestion in the Calvin and Reynolds studies that the asymptotes of the conditioning curves, measured in frequency of CR's in blocks of trials, reach different levels under the different degrees of massing of trials.

In addition to these studies of this variable other investigations have provided evidence for the effectiveness of spacing the trials in conditioning. Thus, Schlosberg (11) reported that 25 trials in a half-hour session established superior conditioning of the withdrawal response to shock in the white rat than 200 trials given in the same period. A number of studies (3, 7, 12) have also reported a decrement in the curve of conditioning with long continued reinforcement under massed conditions.

Purpose of the Experiment

The purpose of the present investigation was to study the effects of varying the inter-trial interval on the rate and limit of acquisition of the conditioned eyelid response in human subjects using a greater number and longer range of intervals than had previously been used. It was originally intended to employ six different inter-trial intervals ranging from six seconds to 120 seconds but difficulties in the apparatus required that the study be limited to four intervals, 9, 15, 30 and 90 sec.

Experimental Technique

Apparatus

The response measured was the change in electrical potentials occurring on the movement of an eyelid. The electrical potentials were picked up by placing two electrodes at the outer limit of the right eye—one below the eyebrow and one just below the eye itself. A schematic diagram of the apparatus is given in Fig. 1. The subject was seated in an electrically shielded room and the electrical potentials were led successively through a pre-amplifier and amplifier to a Grass ink writer (B) and a level selector circuit which activated an electro-magnetic marker (C). This marker operated when the amplified current reached a constant predetermined level, arbitrarily chosen at nine volts. Because of the greater reliability of reading, the responses reported in the present experiment are in terms of the level indicator marker. Comparison of the conditioning scores with those of other studies under comparable conditions suggests that the measure was a relatively insensitive one. This was partly due, of course, to the high value (nine volts) selected for activating the level indicator. In effect this selection was comparable to deciding that a movement of the lid of, say, five mm. in extent had to occur before a response would be counted as occurring rather than some smaller value, e.g., two mm. The selection and, hence, definition of the response measure is, of course, a purely arbitrary matter. The only criterion as to the best manner of de-

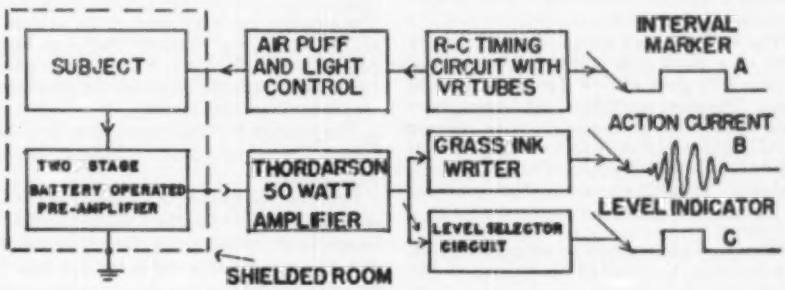

Fig. 1. Schematic diagram of the apparatus.

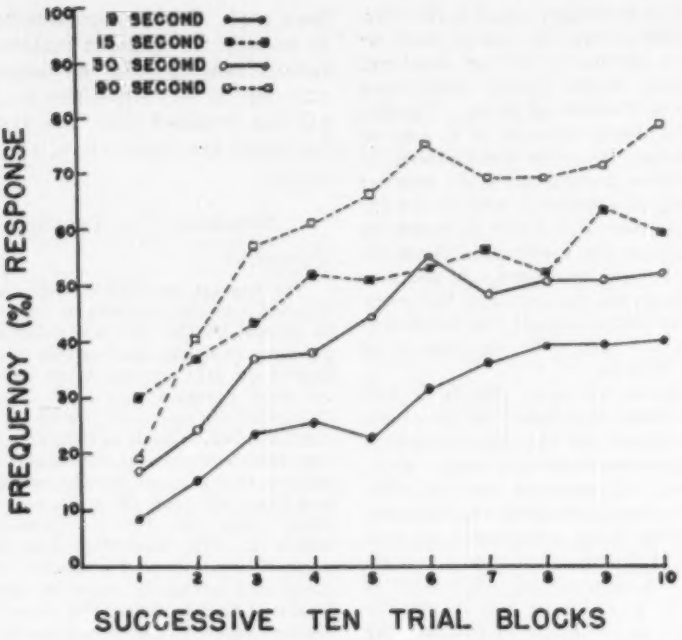

FIG. 2. Curves of acquisition of the conditioned eyelid response in terms of the percentage of CR's occurring in successive blocks of 10 trials.

fining a measure is the pragmatic one of whether it leads to laws.

The time interval between the onset of the CS and the UCS was controlled by means of a thyratron RC timing circuit using voltage regulator tubes to determine the exact firing time. The onset of the stimuli was recorded on the polygraph paper through a third channel by means of an electromagnetic marker.

The amplifying equipment consisted of a two-stage push-pull battery operated preamplifier[1] and a standard Thordarson T 31 W 50-watt audio-frequency amplifier.

The subjects were seated about 75 cm. in front of a black wall surface with a six-cm. circular milk glass window directly in front of them. The room was lighted by a 60-watt overhead light. The CS consisted in an increase of the brightness of the circular milk glass window from a pre-stimulus value of .05 to 4.58 millilamberts. The UCS was a puff of air, produced by the fall of a column of mercury through a distance of 100 mm., delivered to the right cornea. The interval between the CS and the UCS was 600 msec. and a conditioned response was defined as one occurring 250 msec. after the CS and before the UCS.

[1] Details of the preamplifier are not described here but may be obtained by writing to the Department of Psychology, State University of Iowa.

PROCEDURE

The experiment was so designed that four groups of 15 subjects each were run under the same conditions, except for different time intervals between the successive conditioning trials. The intervals employed were on the average 9, 15, 30, and 90 sec. Actually they were varied randomly from 6–12 sec. for the 9-sec. group, 10–20 for the 15-sec., 20–40 for the 30-sec. and 60–120 for the 90-sec. group.

The subjects were volunteers from the Introductory Psychology course at the State University of Iowa.

One hundred paired presentations of the CS and UCS were given in the conditioning period. Except for the 90-sec. group, the trials were presented in one session, varying from 15 min. for the 9-sec. group to 50 min. for the 30-sec. group. The 90-sec. group was given two 75-min.

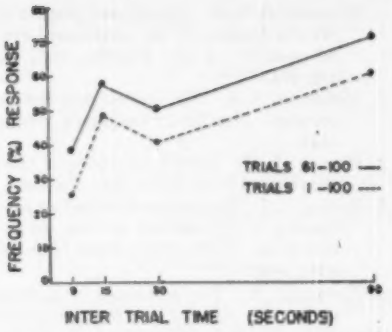

FIG. 3. Curves showing the relationship between amount of conditioning and the inter-trial interval. The upper curve shows the percent frequency of CR's for trials 61–100, the lower curve the percent frequency for all trials (1–100).

sessions on two successive days—50 trials each session.

RESULTS AND DISCUSSION

Acquisition curves in terms of the percentage of conditioned responses occurring in successive blocks of 10 trials are shown in Fig. 2. It will be seen that the curves all have pretty much the same negatively accelerated form and that they tend to have approached an asymptote. It will also be noted that the 9-sec. group was consistently below the other three groups at every point on the curve and that, with the exception of the first 10-trial block, the 90-sec. group was consistently above the other three groups.

The hypothesis that there is no difference between the 9- and 90-sec. groups was tested by means of the Mann-Whitney U-test for the results in terms of frequency of responses obtained in trials 51–100 and 1–100. For trials 51–100 the hypothesis can be rejected at the .4 percent confidence level and for trials 1–100 at the .18 percent level. No other differences between any two of the groups were significant by this test at the one percent level.

The upper curve of Fig. 3 shows the percent frequency of CR's for trials 61–100 and the lower curve the percent frequency of CR's for all trials (1–100) plotted as a function of the different inter-trial intervals. Both curves suggest that the amount of conditioning as revealed by frequency measures is a negatively accelerated, increasing function of the time interval between trials.[3] Presumably this function also approaches a limit suggesting that increase in the inter-trial interval results in no further improvement in conditioning. In an experiment conducted in the Iowa laboratory concurrently with this one, Baron (1) found evidence which suggested that the limit of this function for the human eyelid response was somewhere between 120 and 270 sec. At least he reported no significant difference in the amount of conditioning obtained with groups trained under these two inter-trial intervals, although the 270-sec. group was very slightly superior.

The results of this study are in line with the type of hypothesis that assumes some factor of work decrement (inhibition, fatigue, refractory phase, etc.) in response. The longer intervals between trials allow this decrement to dissipate. Massing of trials leads to a more rapid and greater accumulation of it with a consequent slower rise of the conditioning curve to a lower asymptote.

SUMMARY

Conditioning of the eyelid of human subjects was carried out under four inter-trial time intervals, 9, 15, 30 and 90 sec. on the average. The re-

[3] A plotting of rough estimates of the asymptotes of the learning curves (as a function of inter-trial time interval) also reveals a similar negatively accelerated function.

sults for both the last part and the whole of the conditioning period indicated that the amount of conditioning increased with increase in the magnitude of the inter-trial interval. The data suggest that the relationship, when determined more precisely, will be a negatively accelerated one approaching an asymptote.

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TRANSFER OF TRAINING TO A MOTOR SKILL AS A FUNCTION OF VARIATION IN RATE OF RESPONSE [1]

BY KATHERINE E. BAKER AND RUTH C. WYLIE

Connecticut College

AND

ROBERT M. GAGNÉ

Air Training Command Human Resources Research Center [2]

Introduction

This experiment is a study of the relationship between amount of transfer of training and degree of resemblance between training and final tasks which involve identical stimuli, but require responses which differ to various extents along the dimension of rate of movement.

It has long been recognized by both laymen and psychologists that training on one task will benefit the learning of the second task when the two are similar in some respects. However, there is available no satisfactory, conclusive statement of the relation between the kinds and extent of variations in tasks and amount of transfer obtainable.

The Wylie hypothesis (7) distinguishes between variations in stimuli and in responses, and states that positive transfer occurs if the organism is required to learn to give an 'old' response to a *'new' stimulus*; while on the other hand, negative transfer occurs when the stimulus remains the same and a *'new' response* is required. On the basis of a review of experimental evidence, McGeoch has enlarged upon the first part of the hypothesis to conclude, "Not only is learning to make an old response to a new stimulus a condition which favors positive transfer, but the amount of that transfer is a function of similarities among associated terms" (5, p. 416). Although difficulties have arisen because of the lack of definition as to just what is meant by 'similarities' among stimuli, there seems to be little controversy as to the essential truth of the above statement. Gibson (2) and others (1, 6) have applied the concept of the generalization gradient to account for the positive transfer obtained when a task requires a response to a stimulus which is similar to the pre-training stimulus. According to this hypothesis, during the learning of a task, response tendencies are established not only to the actual stimuli of the task, but also to other stimuli which are similar. Thus, at the beginning of training on a second task, response tendencies to the new stimuli are already present, the strength of these tendencies depending upon degree of similarity of the stimuli.

As for the effects of response variation with stimuli held constant, there is considerably less agreement concerning transfer predictions. In the first place, ambiguities have arisen because there has been no adequate definition as to what constitutes a 'new' response. Osgood (6) seems to imply that 'newness' or variation of response should be defined in terms of the goal achieved. For example, although Macfarlane (4) found positive transfer from running a maze to swimming through it, Osgood claims that these results do not contradict the Wylie hypothesis, since, despite obvious variations in type of body movements, no new response was involved. Even if this way of defining 'newness' of response is accepted, the amounts of positive transfer obtained in bilateral transfer experiments

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[2] Perceptual and Motor Skills Research Laboratory, Lackland Air Force Base, San Antonio, Texas.

cannot be explained in terms of the Wylie hypothesis. If it is claimed that no new response is involved, one should expect equal facilitation of left hand performance from practice by either the left or right hand. This is not the typical result of such experiments. On the other hand, if one assumes that response variation is involved, it must be conceded that the resultant positive transfer constitutes a contradiction to the Wylie hypothesis.

In an attempt to resolve these contradictions, Gagné, Baker and Foster (1) have suggested that a motor 'response' be defined in terms of the particular body member(s) involved, and that such a response be quantitatively described with respect to the direction, force, distance and rate of movement. Any alteration in tasks which involves using different body members or changing the characteristics of the movements of the same body member would then constitute a case of response variation. With this conception, it is obvious that response variations may occur without resulting in negative transfer, and consideration of some variable in addition to 'newness' of response is required for transfer predictions. For the cases in which negative transfer is the result of response variation (e.g., Hunter, 3), it may be seen that *stimulus-response relationships* have been altered in such a way that a response previously associated with one stimulus of the task is now required to be associated with a second stimulus of the task, while the response previously associated with the second stimulus now goes with the first stimulus. If there is a reversal of stimulus-response relationship, a maximum amount of negative transfer is to be expected, since the effects of pre-training now have to be overcome before the correct responses can be made. The conclusion reached by the analysis of Gagné, Baker and Foster is that complete or partial reversal of stimulus-response relationships is the only type of task alteration leading to negative transfer; and that adverse effects of training cannot be accounted for on the basis of either stimulus or response variations alone.

In addition to the problem of defining the term 'response,' experimental evidence is lacking concerning the relation between amount of transfer and degree of alteration of responses. If we assume that *response generalization* is a basic mechanism underlying transfer effects following response variation, then amount of transfer should *decrease* with decreasing similarity of responses. The concept of response generalization is based upon the idea that while a given response is being learned to a stimulus, this stimulus also tends to evoke other responses which are similar to the learned response in force, direction, distance, and rate of movement. Upon beginning to learn a new task which involves identical stimuli but requires a different response, there will already exist some tendency to make this new response, the amount of this tendency depending upon how closely the new response resembles the old. In the absence of alterations in stimulus-response relationships, varying amounts of *positive transfer* are the expected result, just as in the case of stimulus variation. This prediction is to be contrasted with that of Osgood, who states, "Where stimuli are functionally identical and responses are varied, negative transfer and retroactive interference are obtained, the magnitude of both decreasing as similarity between responses increases" (6, p. 135).

The purpose of the experiment reported here was to explore further the transfer effects of response variation when the stimuli of the initial and final tasks are identical, when no stimulus-response reversal is involved, and when responses are varied along the dimension of rate.[1] The task required the subject to follow an irregularly moving stimulus with a pointer controlled by a small hand crank. The apparatus employed allowed for variation in the rate of cranking response required for accurate tracking of the

[1] Variation of extent and perhaps of force of movement accompany rate changes in the present motor task.

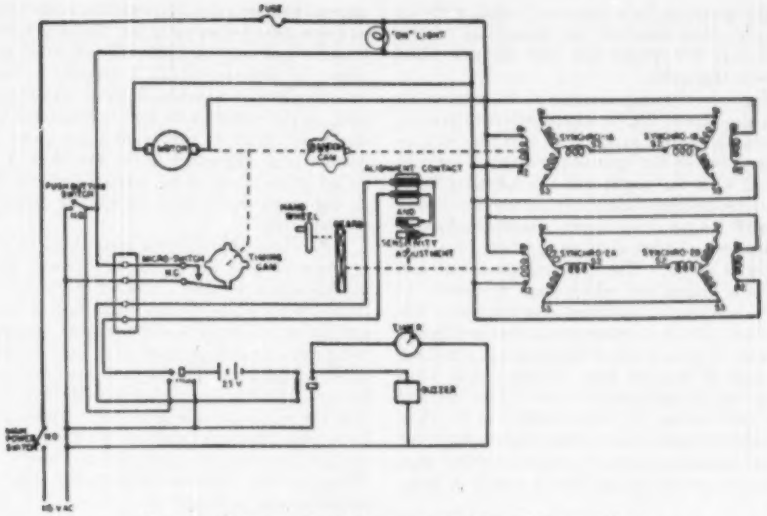

FIG. 1. Diagram of the apparatus showing wiring and the functional relationships among moving parts

moving stimulus. Thus, a subject could be trained on a task requiring one rate of response and then be tested on performance with another rate. In this way, variation in the responses of the initial and final tasks was brought about, though the goal of following the target remained the same. The difference in response rates required in training and final tasks was taken as an index of similarity of response, and amount of transfer was determined by comparing the performances of trained and untrained subjects. Thus, the data of the experiment provide a partial test of the controversial issues cited earlier in the paper.

METHOD

Subjects.—This experiment involved five groups of 31 male college students who volunteered to serve as subjects at the rate of 60 cents per hour. Each group was matched with each other one on the basis of scores on two one-min. trials on the apparatus used in the experiment. The rate of response required in the matching trials was an intermediate one and differed from any of the response rates used in the experiment proper. The correlations between the matching score and the scores on the first four trials at the various rates used in the experiment ranged from .71 to .84, and the lowest of these was used as an estimate of the degree to which matched subjects possessed the same amount of ability to perform this task.[4]

Apparatus.—In general, the apparatus provides for presentation of a target which moves in an irregular fashion over a 120° sector of the circumference of a circular dial; and for a hand crank mechanism for following the target with an index line which also may be moved around the dial. The target consists of a small red diamond upon which is inscribed a white line, ½ in. long and 5/100 in. wide. The white following line, which is moved by means of a three-in. ball-crank handle, is ¼ in. long and 5/100 in. wide. It bisects another diamond, outlined in white lines, which is placed just inside the path of movement of the target. Both the target and the following line appear on the 6½-in. diameter, black face of a bearing indicator of the type commonly in use in radar and sonar equipment. The bearing indicator is set into the tilted front surface of the apparatus box, while the hand crank is directly below on a vertical section of the box. A given pattern of movement of the

[4] Mean scores on the matching criterion for the five groups ranged from 33.85 to 33.92 seconds; standard deviations ranged from 4.00 to 4.15.

target continues for a one-min. period, while an electric timer measures the proportion of this time that the subject has kept the two white lines in alignment.

Fig. 1 is a wiring diagram of the apparatus, and also shows the functional relationships between the various parts. The two pairs of synchro motors at the right of the diagram are connected with the target and the following lines, and serve to bring about their movements. Synchro-1B moves the target diamond through movements of a 'random' cam, driven by a synchronous motor. The synchronous motor also drives a timing cam which stops movement of the 'random' cam and target after one min. The 'random' cam is so constructed that a different pattern of movement of the target is produced for each of the first four one-min. trials, after which the four patterns are repeated for the next four trials during the next rotation of the cam. A red pilot light which is mounted on the front of the apparatus serves to tell the subject that the apparatus is 'on' and that a trial is in progress.

Upon rotation of the shaft of Synchro-2A, Synchro-2B moves the four-in. diameter center disc of the bearing indicator upon which the following line is drawn. This movement is brought about by rotation of the hand crank which is connected with Synchro-2A through a set of gears. The apparatus is provided with interchangeable gear combinations giving five different gear ratios for varying the speed of movement of the following line relative to the rate at which the hand crank is turned. The rates may be designated in terms of the number of turns of the hand crank required to produce a 12.5° movement of the following line. In the present experiment the rates are $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, and 4 turns per 12.5°. Although rate of movement of the following line may vary, its direction of movement is always the same as that of the hand crank. A differential commutator arrangement provides for an electrical contact to be made when target and following lines are in alignment. For the present purposes, the following line made contact and was recorded as being 'on' the target over a range of one mm. on either side of 'true' alignment. A separate circuit containing a buzzer and the clutch of a standard electric timer (measuring to the nearest 0.01 sec.) is connected with the main circuit so that during any given one-minute trial, alignment of the two lines is signalled by the sound of the buzzer, and the time during which alignment is maintained cumulates on the timer.

Procedure.—The subject was seated in front of the apparatus and given the following information about the task. When the apparatus is turned on for a trial the red signal light is lighted and the red diamond moves irregularly throughout a distance of 60 degrees on either side of top center of the dial. The job is to keep the target line and following line in alignment during all the time the red diamond is in motion. This is done by turning the crank handle to the right to make the following line go to the right and vice versa. Whenever these lines are satisfactorily aligned a buzzer sounds to inform the subject. The light turns off at the end of each trial and a one-minute rest precedes the next trial.

Before the training task was undertaken, two trials were given with the gear ratio providing a rate of response of one turn of the crank handle per 12.5° movement of the following line. The number of seconds of alignment for these two one-min. trials was the basis for matching the five experimental groups.

After these matching trials, the gear ratio was changed, and the subject was required to perform the pursuit task for eight one-min. trials at the new rate. The subject was informed that somewhat different amounts of movement would be required than in the previous two trials.

Upon completion of the eight trials of training, a two-min. rest period was given during which the experimenter again changed the gear ratio for the final task. Again the subject was told that the rate of movement to be required had been changed.

TABLE I

SUMMARY OF EXPERIMENTAL PROCEDURE

Group	Training Task Rate		Final Task Rate		Experimental Conditions Investigated
	Rate (No. of Turns/12.5°)	Rate Code No.	Rate (No. of Turns/12.5°)	Rate Code No.	
A	$\frac{1}{4}$	1	$\frac{1}{2}$	2	1_0 and 2_1
B	$\frac{1}{4}$	1	4	4	1_0 and 4_1
C	$\frac{1}{2}$	2	$\frac{1}{4}$	1	2_0 and 1_2
D	2	3	$\frac{1}{4}$	1	3_0 and 1_3
E	4	4	$\frac{1}{2}$	1	4_0 and 1_4

Except for the matching trials, each of the five groups was subjected to a different experimental procedure as shown in Table I. The table indicates the rate of response required in the training and final tasks in terms of the number of crank handle turns per 12.5° movement of the following line, and in terms of the code numbers which we have assigned to these rates. The final column shows the experimental conditions investigated with each group. Again the code numbers for the different rates are used, but with subscripts denoting the number of the rate on which training was given. Thus, it may be seen that the experimental design provides data on the performance at each rate without practice (except for matching trials), and these scores may be compared with performance after training on different ones of the rates. Specifically, performance on the slowest rate without practice (Rate 1_0) may be compared with performance at this rate following training on the other decreasingly similar rates (experimental conditions denoted by Rates 1_2, 1_3, and 1_4). Scores at Rates 2_0 and 4_0 may be compared with performance at these rates following training on the slowest rate (Rates 2_1 and 4_1).

Results

Sign of transfer.—Table II presents the means and standard deviations of time scores obtained under the conditions of the experiment. Each mean is the average number of seconds of alignment out of a possible 240 seconds for four trials. Inspection of the table reveals that the scores made at Rates 1_2, 1_3, 1_4, and 2_1 are higher than scores made at these rates without training (Rates 1_0 and 2_0). These differences, showing positive transfer, are significant at the .05 level or better for both sets of four trials. In one case, Rate 4_1, training produced no significant effect. However, in no case has the type of response variation used here produced negative transfer; and positive transfer is the general result of these task alterations requiring a new response to be given to an old stimulus.[5]

TABLE II

EXPERIMENTAL DATA SHOWING PURSUIT PERFORMANCE AT VARIOUS RATES WITH AND WITHOUT TRAINING

Means and standard deviations of time scores are presented for four response rates without training and for three rates following various kinds of training. The mean for each set of four trials is the average number of seconds of alignment out of a possible 240 seconds. For rate notations, see Table I.

Experimental Conditions	*N*	Trials 1–4		Trials 5–8	
		Mean	SD	Mean	SD
Rate 1_0	62	150.46	15.82	155.34	15.89
Rate 2_0	31	151.51	13.36	155.65	16.97
Rate 3_0	31	143.45	17.81	150.22	17.88
Rate 4_0	31	117.12	27.90	129.35	25.01
Rate 1_2	31	162.72	16.69	165.99	14.71
Rate 1_3	31	155.23	15.74	160.58	15.95
Rate 1_4	31	158.18	16.75	164.15	17.38
Rate 2_1	31	159.55	17.16	161.19	18.05
Rate 4_1	31	114.39	20.94	127.37	24.19

Response similarity and amount of transfer.—Our primary interest in the experiment concerns the relationship between the amount of transfer and the degree to which training and final tasks resemble each other in terms of the responses required. In the present case, tasks were varied in such a way that training and final tasks required different rates of response, and measures of the response rate provide an index of response similarity.

The design of the experiment allows us to compare the effectiveness of training on each of three tasks in which the required response rates are increasingly faster and decreasingly similar compared to the response rate of a common final task. Fig. 2A shows the amounts of transfer to the performance at Rate 1 resulting from training on Rates 2, 3, and 4. As the measure of transfer, we have used the percentage increase in time scores for Rates 1_2, 1_3, and 1_4 over the score found for Rate 1_0. Training at each

[5] It should be remembered that the variations in responses did not involve reversal of stimulus-response relationships.

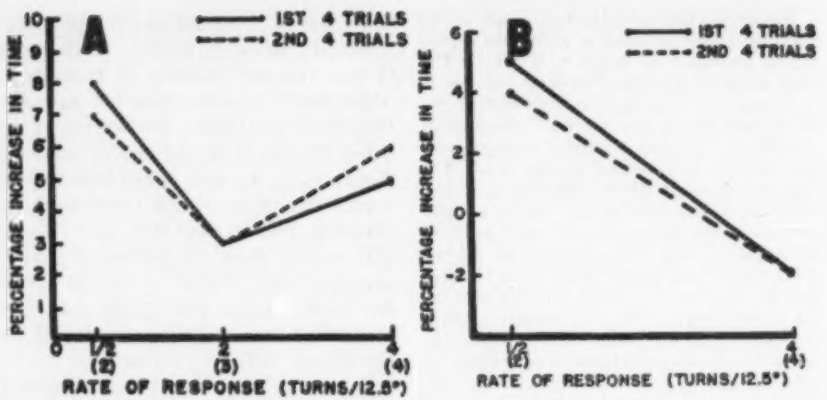

FIG. 2. A. Transfer as a function of similarity of response rates between training and final tasks. The rates of response required in training tasks are indicated along the abscissa. One group was trained at each of these rates and then was tested at Rate 1. Each datum point is the percentage increase in time score after training over the score at Rate 1 for subjects who had no training. Data are presented for two successive sets of four one-min. trials.

B. Transfer from training at one rate to performance at two decreasingly similar rates. Each group was given training at Rate 1 and was then tested at either Rate 2 or Rate 4. Scores after training were compared with scores attained at these rates without training, and each datum point is the percentage increase in time score following training. Data are presented for two successive sets of four one-min. trials.

of the rates produced a significant amount of transfer throughout the two sets of four trials; and, further, training at Rate 2 (most similar to Rate 1) is significantly better than comparable training at Rate 3. The amount of transfer produced by training at Rate 4 (most dissimilar to Rate 1) is not significantly different from the amount following training at Rate 3, but is less than the amount produced by training at Rate 2.[8] Thus, the data of Fig. 2 show that the amount of positive transfer produced by training decreases with decreasing similarity of response rate up to a training task rate which is eight times as fast as the final task rate. A further decrease in similarity did not affect the amount of transfer obtained. (Rate $4 = 6 \times$ Rate 1.)

Direction of response variation and the similarity-transfer relationship.— We have just seen the relationship between amount of transfer and degree of resemblance of responses, when the training task response rates were always faster than the rate required in the final task. The experiment also provides data for the investigation of this relationship when the direction of response variation is reversed, and the training task rate is *slower* than the final task rate. Two groups were given training on Rate 1, after which one proceded to learn the task at Rate 2, while the other group went on to Rate 4. The percentage increase in time scores for Rate 2_1 and 4_1 over the scores for Rates 2_0 and 4_0 meas-

[8] All differences called significant yielded critical ratios at the .05 level or better except the difference between Rates 2 and 4 which is significant at the .07 level. In each case, sets of four trials are considered as a whole, since difficulty of tasks varied from one trial to the next within a set of four. Our statements, therefore, should be considered as specific to such a measure.

ures the degree to which training on Rate 1 transfers to the other rates which are faster and represent decreasing similarity of response rates. Fig. 2B presents these data in graphic form. Training on Rate 1 produces a significant amount of transfer to Rate 2 (which closely resembles Rate 1); but performance on Rate 4 (which is more dissimilar to Rate 1) is unaffected by the training.[7] Thus, the data of Fig. 2B indicate that the same relationship of decreasing transfer with decreasing response similarity holds for either direction of response variation.

Direction of response variation and amount of transfer.—We have seen that the same relationship between relative amounts of transfer and similarity holds regardless of whether the training task rate is faster or slower than the final task rate. However, a comparison of Figs. 2A and 2B reveals the fact that the actual amounts of transfer differ, depending upon the direction of response variation. The difference in rate between Rates 1 and 4 is the same for either direction of variation; but when training is given on Rate 4 (the faster rate) about six percent transfer to Rate 1 is obtained; whereas zero transfer to Rate 4 follows training on Rate 1 (the slower rate). However, comparison of Rates 1_2 and 2_1 shows that the difference in scores is not significant (CR = 1.03). Thus, a small difference in rate does not indicate the same dependence of amount of transfer upon direction of response variation. Response similarity appears to be an adequate basis for predictions of *relative* amounts of transfer to be expected. However, the *actual* amount of transfer obtainable is not predictable on the basis of response similarity alone. Our data allow for the identification of an additional parameter, direction of response variation, which operates to determine the actual amount of transfer obtainable, at least for larger differences in response rate.

Difficulty variation accompanying task alterations.—Since we have scores for each of the rates without preliminary training we may compare the tasks as to their difficulty. It is of interest to investigate this aspect of the tasks to see if difficulty varies with the type of task alteration used here, and to see if a relationship may be found between amount of transfer and difficulty characteristics of tasks. It should be remembered that the operations employed in our task alterations involved changing the ratio of hand crank movement to following line movement. Such an operation changes the requirements of the tasks in a measurable way with respect to rate of response, but also changes the response with respect to extent of movement and perhaps force of movement. Therefore, difficulty variations accompanying task alterations need not be determined solely by response rate variation, and considerations of difficulty may reveal the effects of rate or other variations.

Fig. 3 is a plot of the mean time scores as a function of response rate requirements of the tasks. It may be seen that Rates 1_0 and 2_0 show about the same level of performance, while Rates 3_0 and 4_0 are increasingly more difficult, and lower time scores are attained. Thus, task alterations do produce difficulty changes, but only for the faster rates. All differences except between Rates 1_0 and 2_0 are significant at the .05 level or better.

If we return to Figs. 2A and 2B we see that the greater amount of transfer is obtained for Rates 1_2 and 2_1,

[7] The difference between Rate 2_0 and 2_1 is significant at the .01 level for trials 1–4 and at the .05 level for trials 5–8.

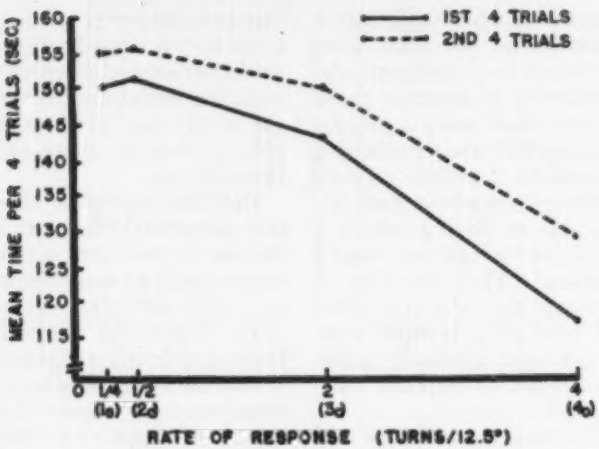

FIG. 3. Difficulty of tasks as a function of required rates of response. For each task requiring a different rate, mean time scores attained without preliminary training are presented for two successive sets of four one-min. trials.

where there is no difference in difficulty; while a smaller amount of transfer occurs for Rates 1_3, 1_4, 4_1, where training and final tasks do differ in difficulty. We may say, then, that when task alteration involves a variation in difficulty of tasks, a smaller amount of transfer is expected than for the case when no difficulty change occurs. It should be noted that all transfer measures are calculated relative to the particular task's initial level of difficulty and, therefore, transfer measures do not include changes in scores due to difficulty differences among original tasks.

It is also of interest to see if there is a relationship between difficulty variation and the finding that the direction of response rate variation affects actual amount of transfer. It was seen that, for large differences in rate, more transfer occurred from training tasks requiring a rate which was faster relative to that of the final task than was obtained from training at slower response rates followed by final tasks requiring faster rates. If we compare the amounts of transfer for Rate 4_1 and for Rate 1_4, we see that more transfer occurs when the individual goes from a difficult task to an easier one. Rates 1_3 and 2_1 do not show a significant difference in amount of transfer, and we fail to show any difficulty difference between these two tasks. Thus, within the limits of the present data the actual amount of transfer obtainable appears to depend upon a difficulty factor in such a way that less transfer is obtained over a given difficulty difference when the direction of task alteration is from easy to difficult or the training task is of a given level of difficulty.

DISCUSSION

In this experiment positive transfer resulted when the training and final tasks involved stimuli which were identical, but responses which differed in rate. Over a given range of differences in response rates, the amount of transfer decreased with decreasing degrees of similarity between training and final tasks. Beyond this degree

of difference in response rates, no further increments in transfer were obtained. Although the *relative* amounts of transfer varied with degree of similarity regardless of whether the training rate was slower or faster than that of the final task, the actual amount of transfer obtained did seem to depend upon this factor of direction of response variation, when large differences in rate were involved. In addition, it was found that less transfer was obtained when there was a difference in difficulty between initial and final tasks.

The first point of theoretical interest here is the fact that positive rather than negative transfer appeared following this kind of task variation. If one is willing to say that our procedure involved the use of 'new' responses, the evidence clearly contradicts those hypotheses which predict negative transfer following response variation, and bears out the predictions made on the basis of response generalization theory. Since the stimuli were obviously identical, any claim that responses were not varied would mean that all aspects of the task were identical; and one is faced with the dilemma of explaining how one could obtain varying amounts of transfer from these functionally identical tasks.

Another point of theoretical interest is the decrease in amount of transfer with decreasing response similarity. According to the response generalization theory there should be more tendency to make responses closely similar to a learned response than to make less similar responses. Thus, training on a given response should facilitate the subsequent learning of a very similar response more than the subsequent learning of a less similar one. Both the amount and sign of the transfer obtained here are consistent with this response generalization theory.

Certain aspects of our results do not permit definite theoretical analysis at present. Thus, although the relative amounts of transfer obtained were consistent with our predictions, it is to be noted that, for larger differences in rate, the actual amounts of transfer depended upon whether initial training was given at a fast or slow rate. This is seen by the fact that more transfer occurred from Rate 4 to Rate 1 than in the reverse direction. Response generalization theory in its simplest form is not adequate to account for these results. However, we might suggest that either the level of difficulty of the training task or the direction one moves along the difficulty continuum when going from initial to final task might be a factor in determining the amount of transfer. If either of these was the explanation, we should get no more transfer from Rate 2 to Rate 1 than when we go in the reverse direction, since Rates 1 and 2 seem to be at the same difficulty level; but we should get more transfer from Rate 4 to Rate 1, than vice versa, since these rates differ in difficulty. Actually we do get more transfer from Rate 4 to Rate 1 than we do from Rate 1 to Rate 4, whereas we get the same amount of transfer in either direction between Rates 1 and 2. Thus, we know that level of difficulty or the direction one moves along the difficulty continuum in going from initial to final task, or both, are factors in determining actual amounts of transfer. It would be impossible, within the limits of our data, to choose between these two factors. Thus, we see that the only consistent, identifiable relation is that between rate differences and relative amounts of transfer, this relation being explainable in terms of response generalization theory. Further experimentation is necessary in order to determine why more transfer should occur when going from a faster to a slower rate over a large, constant rate difference.

Several precautions should be taken in interpreting the data of the present experiment. First of all it should be noted that, although our plan involved varying responses along the dimension of rate, a number of concomitant unmeasured vari-

ations undoubtedly occurred. Since it is impossible to vary rate without varying some of these other aspects of the response, it is impossible to say that rate variations alone would produce the results found here. Neither can we say to what extent, if any, the other varying aspects of responses, taken separately, would influence transfer results. Our assumption was, however, that if a judged similarity scale of these tasks were to be determined, the tasks would rank in the order of measurable rates. Perhaps the sensed difficulty of the task, or other unmeasured inter-task differences would contribute to the judgments of similarity; but we have assumed that such factors would not have changed the order of judged similarity from that primarily determined by rate differences. However, we do not claim that the rate differences are anything more precise than an ordinal measure of the degree of similarity which these tasks had for our subjects. This kind of measure does not permit an exact statement of the function relating amount of transfer to degree of similarity of tasks. Nevertheless, the trends of the data are quite clear.

It is also important to note the characteristics of the task and the kind of measure of learning employed here, since many of the results and implications might differ from the present ones had we employed a slightly different task or used some other measures of learning. The response required in this task involved two aspects: following the general direction and speed of a moving target; and, at the same time, maintaining precise alignment of two fairly small stimuli. This response differs from certain other pursuit or tracking tasks which involve a general following reaction, but require alignment of pursuit and target stimuli at only one point, after the target stimulus has been brought back from an excursion away from this point. In our task the subject receives no credit for following the general speed and direction of the target, but scores only during the time when exact alignment is being maintained. If we had measured errors in terms of the number of times the subject moved off the target, we might have found different transfer effects and optimum rates for training. In the present case it is conceivable that two individuals might be on the target for the same total amount of time, yet one might achieve this total while vacillating on and off the target frequently, while the other might make fewer errors but spend longer periods of time on and off the target. The learning measure employed provides what is perhaps the most practically important datum, i.e., the amount of time on the target; but we have no way of determining the extent to which such a measure reflects number and degree of error in performance, which also may be of theoretical and practical interest.

Summary

An experiment is reported which deals with transfer of training as a function of differences in response rate between the training and final tasks.

The tasks employed in this experiment involved turning a crank so as to keep a pointer in alignment with a target which moved irregularly back and forth over a 120° sector on the circumference of a dial. Various gear combinations could be installed in the apparatus, making it possible to vary the ratio of rate of crank movement to rate of pointer movement. The response rate required for accurate pursuit of the target was measured in terms of the number of crank handle turns needed for a 12.5° movement of the pointer. The apparatus provided for four tasks requiring different response rates: Rate 1 = ¼ turn per 12.5° pointer movement; Rate 2 = ½ turn per 12.5°; Rate 3 = 2 turns per 12.5°; and Rate 4 = 4 turns per 12.5°. The measure of performance was the number of seconds during which alignment was successfully maintained during a one-min. trial.

The experiment involved 155 subjects divided into five matched groups.

Each group was given eight training trials at one rate followed by eight trials of a final task requiring another response rate. Combinations of training and final tasks were given which permitted us to make the following comparisons: (a) Scores made at Rate 1 without training vs. scores made at Rate 1 after training at Rates 2, 3, or 4 (which were decreasingly similar to Rate 1); (b) Scores made at Rates 2 and 4 without training vs. scores made at Rates 2 and 4 after training at Rate 1 (which was more similar to Rate 2 than to Rate 4).

1. A significant degree of positive transfer to Rate 1 resulted from training at Rates 2, 3, and 4. Also a significant degree of positive transfer to Rate 2 resulted from training at Rate 1. Thus, the hypothesis predicting positive transfer from response variation is supported by these results.

2. The relative amount of positive transfer obtained from training depended upon the degree of similarity between the training and final tasks, expressed in terms of resemblance between required rates of response. This was true regardless of whether the training task required a faster response rate than the final task or the reverse procedure was followed. Training at Rates 2 and 3, which were decreasingly similar to Rate 1, yielded significantly decreasing amounts of positive transfer to Rate 1; while training at Rate 4 yielded essentially the same amount of transfer as training at Rate 3. Also, Rate 1 yielded more transfer to Rate 2 than to Rate 4.

3. Although the same similarity-transfer relationship holds when training task rate is faster than final task rate as when training is at a slower rate than the final task, the actual amount of transfer differs for the two conditions. For a large constant difference in rate, training on the faster rate produces more transfer to performance at the slower rate than vice versa. Although there is no more transfer from Rate 2 to Rate 1 than in the reverse direction, training at Rate 4 produces a six percent improvement in performance at Rate 1, while Rate 1 yields zero transfer to Rate 4.

4. It was found that the task alteration used here also resulted in variations in difficulty of the tasks, as measured by the level of performance attainable without training. While Rates 1 and 2 did not differ in difficulty, Rate 3 was significantly more difficult than these rates; and Rate 4 was significantly more difficult than all the other rates. Considering the factor of difficulty as it relates to transfer effects, we see that a greater amount of transfer was obtained when training and final tasks did not differ in difficulty than when such a difference did exist. In addition, for large differences in response rate, a greater actual amount of transfer was obtained when the training task was more difficult than the final task. This effect may be a function of the relative difficulty of the training task compared to the final task, or it may be dependent upon the absolute level of difficulty of the training task.

5. These results were found to be consistent with response generaliztion theory.

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THE EFFECT OF INCREASED POSITIVE RADIAL ACCELERATION UPON DISCRIMINATION REACTION TIME[1]

BY A. A. CANFIELD, A. L. COMREY, R. C. WILSON, AND W. S. ZIMMERMAN

University of Southern California

The pilot of modern high-speed aircraft is required to make rapid discriminatory responses to a complex of stimuli if he is to avoid mishap. The increased speed and maneuverability of modern aircraft have placed the pilot under greater and greater stress conditions. The present investigation deals with the effect of exposure to one of these stress conditions, that of increased positive radial acceleration.

Pilots of aircraft encounter conditions of increased positive radial acceleration in maneuvers such as turns and recovery from dives. The accelerative force imposed is like that acting upon any body rotating about a fixed point, and this force tends to accelerate the body away from and at right angles to, the arc of rotation. For convenience the accelerative rate that would result, if the body were free to move, is converted into multiples of the normal gravitational accelerative force of the earth. A five-*g* force is then equivalent to five times that of gravity or has an accelerative rate of approximately 161 feet per sec. per sec. The applied force is termed positive, as in this paper, if the force acts in a head to seat direction on the subject. This tends to pool the blood in the lower extremities and the lower trunk region of the body. Negative *g* is just the opposite of positive *g* and the body fluids tend to be pooled in the head and upper trunk regions. Human tolerance to these accelerative forces is higher for positive *g* than for negative *g*, and it is the positive *g* condition that is the most frequently encountered by pilots in the familiar seated position in the aircraft. This research was directed at a determination of the effects of increased positive radial acceleration on discrimination reaction time.

Procedure

Apparatus.—All of the research data included, unless otherwise specified, were obtained on volunteer college students tested on the human centrifuge at the University of Southern California. The centrifuge consists of a boom, approximately 23 ft. long, which rotates in the horizontal plane. *S*s are seated at the extreme end of the boom, and tested while it is in motion. As the centrifuge rotates, the seat swings out approaching a position horizontal with the floor. The speed of rotation of the boom determines the amount of radial accelerative force imposed. Since a relaxed *S* loses vision temporarily at about 4-5 *g*, a condition called blackout, it was necessary in this study to keep the level of force imposed moderately low. The use of the Navy Coverall Type Z-2 anti-*g* suit, however, enabled the use of *g* forces up to five with no symptoms of physiological embarrassment.

The test problems were presented on a panel mounted vertically in front of *S*. He was to move a toggle switch in the direction that a red light lay from a green one on this test panel. A small white light in the center served as a warning light and the test stimuli were presented in from 1.5 to 2.5 sec. after the appearance of this warning light. This foreperiod was varied from response to response in an irregular order. The test panel and rate of presentation of the stimuli were quite similar to that used in the Army Air Forces Psychology Program in the last war (3).

In each experimental trial, as soon as the centrifuge had reached terminal speed, *E* turned on

[1] This research was conducted at the University of Southern California as a part of Navy Contract N6ori77-Task Order III, Neil D. Warren, responsible investigator. Special assistance in the construction of equipment was given by A. Lu Ulberg and Robert Leyden.

the test mechanism. The warning light appeared and was then followed by the test lights. Each experimental trial lasted for approximately 15 sec., during which time *S* made five separate responses. A standard electric timer recorded the cumulative time for the five trials and this cumulated time served as the basic measure of the experiment.

For any one discrimination pattern setting, the red light would be either above, below, to the right, or to the left of the green light. The panel had one red and one green light in each of the four corners. The red light could then appear above or below the green on either the left or right side of the panel. Similarly, the red light might be to the right or left of the green light on either the top or bottom half of the panel. Eight different cables were constructed to be connected to the stimuli-controlling apparatus. Each of the cables presented the stimuli in a different order and in different positions on the panel. These cables were changed on every trial in order to keep *S*s from learning a specific response sequence. Table I shows the stimuli presented by each of the eight cables. The letter on the left in each cell represents the side of the panel on which the pair of lights appeared and the second letter indicates the direction of the correct response movement. For example, Setting No. 1 for Cable No. 1 was such that the lights appeared on the bottom half of the panel and a movement to the right was required. The five settings shown for each cable would go to make up an experimental trial. *S* never knew which cable he was going to receive and, since he received the same cable only once each day and no more than four times during the course of the entire experiment, it is extremely doubtful that any learning of a movement sequence occurred.

Experimental Design.—The 23 *S*s used in the experiment were given medical examinations and indoctrination rides on the centrifuge prior to the experimental trials to determine their physical fitness for participation in the experiment. A three-*g* warmup run, without test problems, was given on each experimental day prior to the experimental trials in an effort to help *S* become adjusted to the centrifuge situation. The directions given prior to the first day's experimental trials were as follows.

> This test was designed to see how quickly and accurately you can react to a pattern of lights. On the board in front of you there are nine lights, two in each corner of the board and one in the center. The light in the center is a white light and lights up before each item in the series to let you know that the other lights are about to come on. In each corner of the board there are two lights; one of them is red and the other is green. In this test, one light will come on in each of two corners. For example this light might come on in this corner and this light in this corner. [*E* points to any two lights that might appear.] Your task is to make a movement with this response button that indicates the direction of the red light from the green. Think of the two lights as representing an arrow. The red light is the head or tip of the arrow and the green light is the tail or feathered end. Read the lights from red to green and then respond as quickly as you can with the response knob.

Following the directions, each *S* was given five practice trials to insure that he understood the directions and was making the correct responses movements. He was shown that when and only when he made the correct response movement, the test lights would go out.

The 23 *S*s were divided into three groups; A, B, and C, with 8, 7, and 8 *S*s respectively in the three groups. Each *S* was given six experimental trials on each of four different testing days. The six experimental trials given on each of the test days were composed of two runs each at 1, 3, and 5 *g*. This gave a total of eight experimental trials at each of the *g* levels for each of the 23 *S*s. Groups A, B, and C were given quite different orders of *g* trials in order that the experimental results obtained would not occur merely because of the use of a particular *g*-level sequence. Each day the *g*-level trials were given in the abcabc order so that each *S* would have one of the three different *g*-levels in the first and last half of the day's trials. Each of the three

TABLE I

ORDER, LOCATION, AND DIRECTION OF STIMULI PRESENTED BY THE VARIOUS CABLES

Cable No.	1	2	3	4	5	6	7	8
Setting 1	BR	RD	TL	LU	RU	BL	LD	TR
Setting 2	RD	TR	LD	BL	BR	LU	TL	RU
Setting 3	TL	BL	RU	TR	LD	BR	RD	LU
Setting 4	LU	RU	BL	RD	TR	TL	BR	LD
Setting 5	LD	LU	TR	BR	TL	RD	RU	BL

g-levels occurred almost exactly equally in every position in the series.

The *S*s did not, however, receive the same cables the same number of times at each *g* level. Since some of the sequences of items on some of the cables might have been more difficult than others, it was considered desirable to determine the comparative difficulty of response for each of the cables. To do this, a separate group of 40 *S*s were tested off the centrifuge with the same apparatus. Each *S* was tested on each cable, the order of presentation being systematically varied, and on the basis of these scores stanine norm scores for each cable were prepared. The procedure was the same as that suggested by Guilford (2) for the C-scale, except that the top and bottom two intervals were combined to give a nine point scale. The times of the original experimental group tested on the centrifuge were then converted into these stanine scores. The score for each experimental trial was based on the dificulty of the particular cable response order as determined from the norm group response times.

Results

The first and second half of each day's trials were exactly alike in terms of the *g*-level order. Each of the three *g* levels used in the study on each day occurred once in the first three and once in the last three trials. For example, one *S* might have had his trials with the *g*-level order of 1, 3, 5, 1, 3, 5; another might have 5, 1, 3, 5, 1, 3; etc. The data obtained on the first half of each day were combined to make up what will be called the first half results and the data obtained on the second half of each day were combined to make up the second half. These two halves were then analyzed separately as shown in Table II. It will be remembered that the mean values represent cumulated time for five responses.

Pearson product-moment correlations were computed between reaction times at the different *g* levels for the two halves separately. These were, for the first half, $r_{13} = .86$, $r_{15} = .74$, $r_{35} = .72$; and for the second half they were $r_{13} = .59$, $r_{15} = .71$, and $r_{35} = .81$.

TABLE II

Mean, Standard Deviation, and Standard Error of the Mean of the Reaction Times of the First and Second Half-Days' Trials

(Number of sec. for five consecutive responses)

g Level	Mean		M
First Half			
1	2.95	.45	.10
3	3.05	.38	.08
5	3.13	.41	.09
Second Half			
1	2.96	.34	.07
3	2.87	.32	.07
5	2.93	.31	.07

These correlations were moderately high and indicate a fairly consistent level of performance by each *S*. Differences in performance at the various *g* levels were then tested by means of the *t*-ratio. The only significant differences that occurred were between the 1- and 3-*g* levels and the 1- and 5-*g* levels in the first half analysis. Response times for the first 3-*g* and 5-*g* trials of the day were significantly slower than they were at one *g*, when all four experimental days were combined. Both the *t*-ratios of 3.55 for the 1–3 *g* comparison and 3.42 for the 1–5 *g* comparison were significant beyond the one percent level of confidence.

The data by halves were then broken down by day and *t*-ratios were computed for the differences between mean reaction time scores by *g* level. Each of the four experimental days was considered separately. This analysis showed a significant difference in performance for only the 1-*g* and 5-*g* comparison on the first and second days for the first half trials. No other significant difference between any

two *g* levels for any day, regardless of the half considered, was found. The *t*-ratios between the 1-*g* and the 5-*g* conditions on the first half of the first day and the first half of the second day were 2.54 and 2.67 respectively. Both of these values are significant at the five percent level of confidence.

Further analysis of the data revealed that while the mean reaction time for the 1-*g* trials for the first half and the second half did not differ significantly, the mean values for the 3-*g* and 5-*g* conditons were both significantly shorter in the second-half trials than they were in the first. The *t*-ratios of 3.17 and 3.74 for the 3- and 5-*g* conditions respectively were both significant beyond the one percent level. With these differences clearly in mind, the two halves were combined as a matter of interest, and no difference between the reactions times of the separate *g* levels was found.

Discussion [1]

The most striking result of the experiment is the fact that the ability to make discriminatory reactions seems to be impaired by increased positive radial acceleration in the first few experimental trials for the first couple of experimental days only, and thereafter is unaffected. No significant differences in reaction time were found on any day for the second half of the day's trials, and even the first half of the day's trials showed no difference on the third and fourth experimental days.

It seems highly unlikely that any physiological compensatory mechanism could account for these results. Instead it is proposed that the effects of novelty, distraction, and apprehension involved in becoming adjusted to the experimental situation account for the reduction in *S*s effectiveness early in the experimental series. After a few exposures to increased *g*, however, *S* becomes adapted and functions just as well at five *g* as he does at one *g*.

A consideration of the results of this experiment and others on Spatial Orientation (4) and Perceptual Speed (1) leads to the conclusion that no drastic changes in mental efficiency occur in the pilot up to the level of five *g*. In none of these three studies has any significant decrease in performance been found with the increased accelerative forces as long as *S* is wearing an anti-*g* suit and the forces imposed are no stronger than five *g*. As far as performance on these tasks is concerned, with the *S* still maintaining vision, the increased weight of the body and the body fluid shifts associated with the imposed forces do not seem to result in any altered performance within a 15 sec. period.

Further research is indicated, however, in lengthening the period of exposure to these forces, in pushing the level of forces applied higher, and in examining other types of human skills associated with piloting an airplane that might be affected.

Summary

This experiment was designed to determine the effects of increased positive radial acceleration on discrimination reaction time. Twenty-three male college students served as *S*s. All experimental trials were given with *S* wearing an anti-*g* suit. The problem was to determine the direction that a red light lay from a green one on an exposure panel and then to make a simple motor response indicating that direction as quickly as possible. A series of four experimental days were given to each *S*. On each of the four days *S*s were tested twice at the normal resting condition (one *g*) and

[1] It is important to keep in mind the fact that the results and discussion presented in this paper consider only the forces applied, inasmuch as instances of blackout and greyout are not included. Naturally, when vision is lost or markedly impaired, this type of response time would be greatly altered.

twice each at positive radial accelerative forces equal to three and five *g*. The major conclusions of the experiment follow:

1. The ability of *S* to make efficient visual discriminatory reactions is impaired significantly under increased positive radial acceleration forces of five *g* for the first few trials early in the history of exposure, but thereafter is unaffected.

2. This impairment, early in the history of exposure, is attributed to distraction, novelty, and apprehension associated with riding the centrifuge rather than to any fundamental physiological embarrassment.

3. The results of this study when taken in conjunction with others performed at this laboratory lead to the conclusion that visual discriminatory processes and certain basic mental abilities are not affected by these forces up to a level of five *g* as long as vision is maintained.

4. Futher research should be devoted to continued study of these and other skills under conditions of lengthened exposure and increased level of force to determine if some critical point, short of unconsciousness, does exist where these functions might be impaired.

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THE PERCEPTION OF THE VERTICAL. IV. ADJUSTMENT TO THE VERTICAL WITH NORMAL AND TILTED VISUAL FRAMES OF REFERENCE[1]

BY GEORGE E. PASSEY

The Tulane University of Louisiana
and
School of Aviation Medicine and Research Naval Air Station, Pensacola, Florida

Introduction

The perception of the visual and postural vertical can be differentiated with respect to their modes of determination. Visual vertical perception involves the adjustment of a visual target to a position aligned with the gravitational vertical. In determining the postural vertical, *S* adjusts his body axis until he feels that it is aligned with the gravitational vertical.

The controversy as to whether postural or visual factors are of primary importance in the maintenance of equilibrium is not a new one. The arguments have been summarized by Gibson and Mowrer (4). On the basis on the available evidence they conclude that in the event of conflict between postural and visual factors the perceived vertical will be more nearly aligned with the gravitational vertical than with a displaced visual framework.

Asch and Witkin (1, 2), using a tilted visual framework and examining the perception of the visual vertical, have presented evidence in favor of the position of Koffka (6), viz., that the main lines of visual space are of most importance in the determination of the vertical. Koffka contended that in event of conflict between the postural factors and visual factors the individual, when asked to determine the vertical and horizontal, would select the main lines of visual space to the neglect of the postural factors.

The determination of the postural vertical in the presence of displaced visual frames of reference has not been systematically attacked and it is the purpose of this study to investigate some aspects of this problem. For purposes of statistical test the following hypotheses were proposed:

1. Adjustment to the vertical will be made to the main lines of visual space in the presence of conflicting gravitational and visual cues.

2. The average error of adjustment to the vertical will be equal for conditions of conflict or conditions of no conflict of the gravitational vertical and the main lines of visual space.

3. The average error of adjustment to the vertical will exhibit no differential effect when *S* and the visual frame of reference are displaced from the gravitational vertical in the same or opposite quadrants. (*S* right, visual framework right versus *S* left, visual framework right.)

4. No differential effect with respect to the amount of divergence between the gravitational vertical and the vertical set by the visual framework

[1] This report is part of a series of investigations conducted jointly with the School of Aviation Medicine and Research under Contract N7onr-434, Task Order I, with the Office of Naval Research, in cooperation with the Bureau of Medicine and Surgery and the Tulane University of Louisiana. Project designation number NR-140-455 of the Medical Sciences Division, Office of Naval Research. Project NM-001-037 of the Research Division, Bureau of Medicine and Surgery. Task Order Director: Cecil W. Mann.

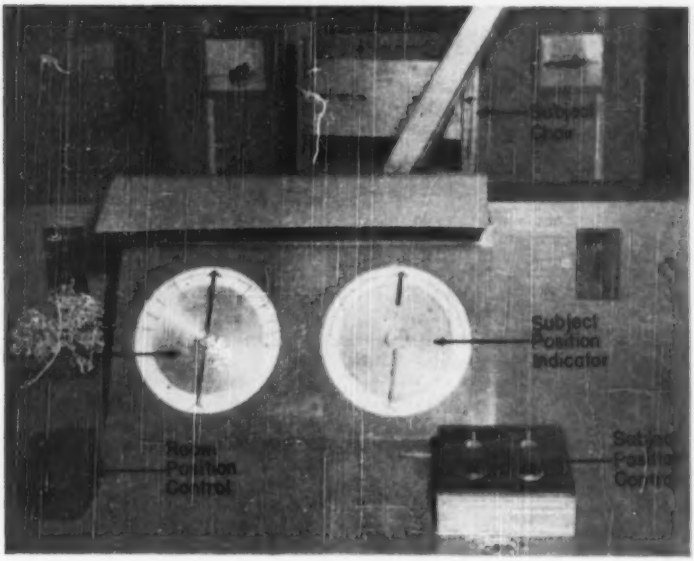

FIG. 1 Navy orientation box from experimenter's station

exists when in adjustment to the gravitational vertical the indices of average and constant error are used.

APPARATUS AND PROCEDURE

The apparatus used in this experiment consisted of a tilting room and chair as shown from *E*'s position in Fig. 1. This equipment was constructed by the Special Devices Center of the Navy Department under the direction of Dr. H. A. Witkin. An experimental environment in the fore and aft planes similar to the present one for the lateral plane was used by Kleint (5). The equipment was enclosed in an air conditioned room which provided constant temperature.

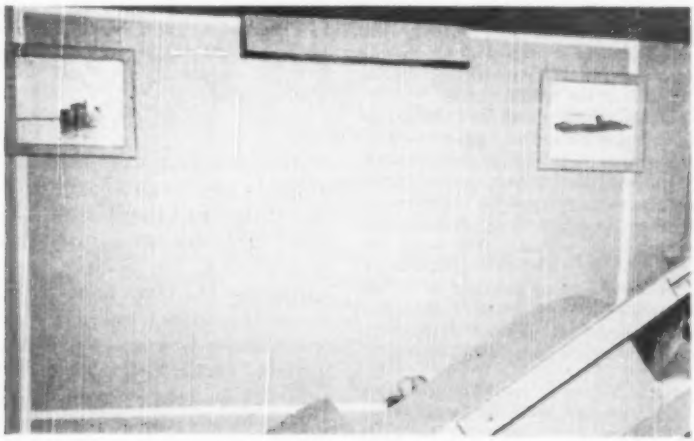

FIG. 2. Navy orientation box showing visual framework available

TABLE I

MEAN CONSTANT ERROR IN DEGREES FOR ADJUSTMENT TO THE GRAVITATIONAL VERTICAL FOR TILTED VISUAL FRAMEWORK

Room and *S* Position		Room Tilt in Degrees of Arc				
		0°	5°	10°	15°	20°
S Right	Room Right	.4R	2.2R	2.3R	2.5R	2.0R
S Left	Room Right	.3R	.7R	1.5R	1.8R	.8R
S Right	Room Left	.4R	.2R	.3L	.8L	.7L
S Left	Room Left	.3R	1.1L	1.1L	1.7L	2.0L
	Room Right (*M*)	.35R	1.45R	1.90R	2.15R	1.40R
	Room Left (*M*)	.35R	.45L	.70L	1.25L	1.35L

The tilting room, an eight-foot cube, was arranged to rotate laterally on an axis which passed through its center. A chair was located inside the tilting room and could be tilted laterally on the same axis as the room. Both chair and room were capable of rotation through 360°. *S* was seated at approximately the center of the room and faced a wall which contained two pictures, a fluorescent light, and a series of lines painted in white which were oriented with respect to the main lines of the room. The entire room was painted in neutral gray. These features are illustrated in Fig. 2. Behind *S* was an uncovered wall which gave *E* a full view of the room at all times.

The room could be moved in either direction at a constant velocity of 225° per min., while the chair could be moved at a constant velocity of 133° per min. In its original form the mechanism provided for adjustment of both the room and chair by *E*. For purposes of this experiment the apparatus was modified to allow control of the chair by *S* in addition. Adjustment to the vertical was measured in degrees by the use of a selsyn remote recording system and determinations were made to the nearest degree.

Five NROTC Midshipmen[2] from the Tulane Unit volunteered as *S*s and each served for a series of 10 experimental sessions of approximately 40 min. duration. During each session *S* made 20 adjustments to the vertical. All adjustments were made from a position of 45° on either side of the gravitational vertical. The room was tilted at 5° right, 10° right, 15° right, and 20° right, and at corresponding positions to the left of the gravitational vertical. Twenty determinations were made for each position of room tilt, 10 of which were made from tilted positions in each quadrant. In addition each *S* made 40 adjustments when the walls of the room were in alignment with the gravitational vertical. Twenty of these adjustments were from an initial tilt to the right while 20 were from positions of initial tilt to the left.

The experimental procedure consisted in bringing *S* into the experimental room and explaining to him that the purpose of the experiment was to see how well he could adjust himself to a position in which he would be sitting upright with respect to the world outside. He was told and shown that both the chair and the room could be tilted independently of one another. He was presented with the upright position of both chair and room following each adjustment. A trial consisted in having *S* close his eyes; following this both *S* and the room were positioned; *S* was then instructed to open his eyes and return himself to the upright. When satisfied with his position he so informed *E*. *E* then instructed *S* to close his eyes following which he was returned by a series of random movements to the upright position. Each session consisted of adjustments from all experimental positions presented in random order.

RESULTS

The data for examination of hypothesis one are presented in Table I. Each entry in Table I is the mean constant error for 50 adjustments. By inspection it can be seen that *S* did not accept the main lines of the visual frame of reference as his vertical when it was not in alignment with the gravitational vertical. On the basis of this evidence we reject hypothesis one.

Hypothesis two can be examined by reference to Tables I and II. Ex-

[2] The author wishes to acknowledge the assistance of Midshipmen W. J. Anderson, P. W. Floyd, A. Jackson, R. C. Kerwath, and E. S. Schlesinger, who volunteered as subjects.

amination of the entries in Table I indicates that in all instances a larger mean constant error results when adjustments are made in the presence of tilted visual frames of reference than when the main lines of the visual framework are aligned with the gravitational vertical. In Table II the data show greater mean average error for adjustments made when the main lines of visual space were not aligned with the gravitational vertical. The variability of the data was checked using the test described by Bartlett (3). The chances are greater than five in 100 that the difference in variability occurred as a result of chance. We reject hypothesis two.

TABLE II

MEAN AVERAGE ERROR IN DEGREES FOR ADJUSTMENT TO THE GRAVITATIONAL VERTICAL FOR TILTED VISUAL FRAMEWORK

Room and *S* Position	Room Tilt				
	0°	5°	10°	15°	20°
S Right Room Right	1.2	2.4	3.1	3.1	2.9
S Left Room Right	1.2	1.9	2.3	2.5	2.3
S Right Room Left	1.4	1.9	2.1	2.2	2.3
S Left Room Left	1.4	1.9	2.2	2.6	2.7
Mean	1.3	2.0	2.7	2.6	2.6

TABLE III

THE *t*-VALUES AND PROBABILITIES FOR CONSTANT ERROR OF ADJUSTMENT TO THE VERTICAL IN PRESENCE OF TILTED VISUAL FRAMEWORK

Room Inclination	Room Right				Room Left			
	Seat Left		Seat Right		Seat Left		Seat Right	
	t	*P*	*t*	*P*	*t*	*P*	*t*	*P*
0° versus 5°	1.58	.10	3.66	.001	4.41	.001	.67	.10
0° versus 10°	3.55	.001	3.21	.01	3.13	.01	2.00	.05
0° versus 15°	4.15	.001	3.23	.01	4.40	.001	1.28	.10
0° versus 20°	1.70	.05	2.34	.05	4.81	.001	1.36	.10

TABLE IV

THE *t*-VALUES AND PROBABILITIES FOR MEAN AVERAGE ERROR OF ADJUSTMENT TO THE VERTICAL FOR TILTED VISUAL FRAMEWORK

Room Inclination	Room Right				Room Left			
	Seat Left		Seat Right		Seat Left		Seat Right	
	t	*P*	*t*	*P*	*t*	*P*	*t*	*P*
0° versus 5°	3.15	.01	3.87	.001	2.27	.05	2.85	.01
0° versus 10°	3.64	.001	4.86	.001	2.92	.01	2.42	.02
0° versus 15°	4.52	.001	3.80	.001	3.67	.001	3.24	.01
0° versus 20°	3.87	.001	3.78	.001	3.17	.01	2.97	.01

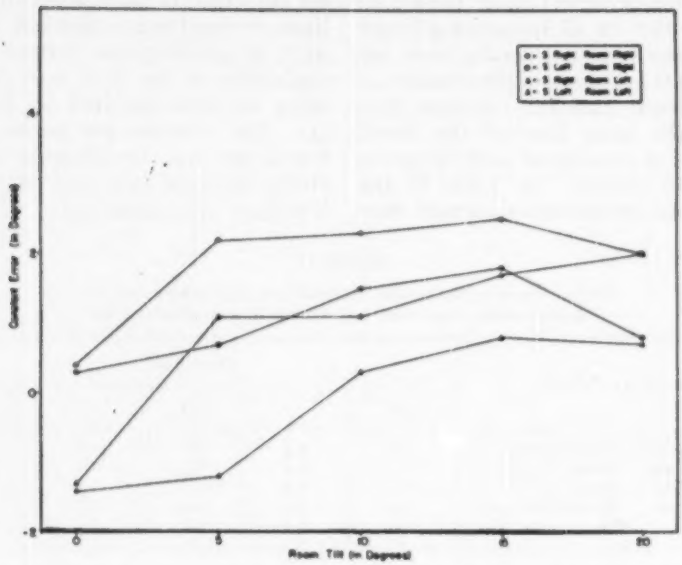

FIG. 3. Constant error in degrees for adjustment to the vertical in presence of tilted visual framework (positive points indicate error in direction of room tilt)

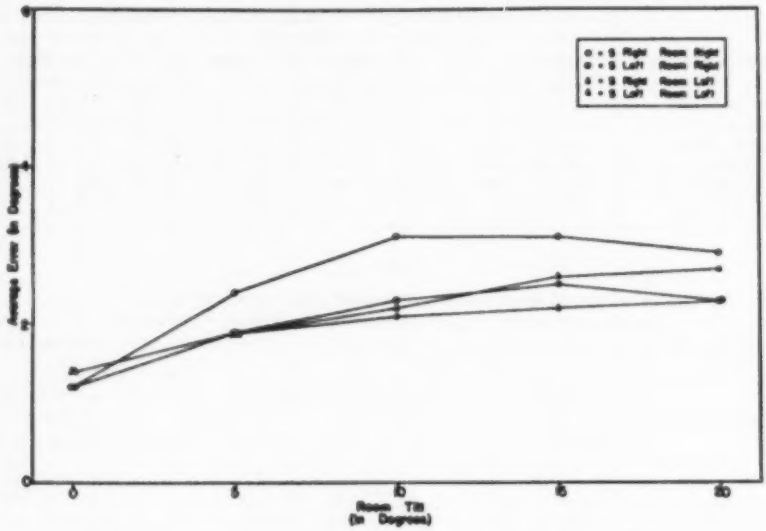

FIG. 4. Average error in degrees for adjustment to the vertical in presence of tilted visual framework

Tables I and II indicate that when adjustments were made from an initial tilt position in the same quadrant as the tilted visual frame of reference both constant error in the direction of the tilted visual framework and average error were greater than when adjustment was made from a position of initial tilt in the opposite quadrant to the tilted visual framework. Reference to Tables III and IV show an indication of this in the larger probabilities of chance occurrence for the differences shown when adjustments were made from positions of tilt in the opposite quadrant to the tilted visual framework. Figs. 3 and 4 illustrate graphically the data of Tables I and II. The variability of data for adjustment from the same quadrant when compared with that of adjustment from opposite quadrant gave a P between .10 and .20. We reject hypothesis three.

Examination of Figs. 3 and 4 shows that as the amount of tilt of the visual framework increases there is an increase in the mean constant error in the direction of the tilted visual framework and an increase in the average error. The only statistically significant differences were between positions of alignment of the visual framework with the gravitational vertical and positions of tilt of the visual frame of reference. These differences as shown by their t-values and probabilities are presented in Tables III and IV. While the probabilities of chance occurrence are not all statistically significant, the trend is clear. We reject hypothesis four.

Discussion

By rejection of hypothesis one we have in effect denied the contention of Koffka that in event of conflict between postural and visual factors the visual factors would be the determiners of the subjectively accepted vertical. In our experiment the main lines of visual space were not accepted as determiners of the vertical. The data of Asch and Witkin working with the visual vertical was interpreted by these authors in support of Koffka's contention. They show only that in the presence of conflicting postural and visual factors that the visual vertical tends to be judged nearer to the vertical set by the visual framework than to the gravitational vertical. They state that a tendency existed because the visual framework was a weak one. The visual framework used in this experiment was, however, the entire visual environment of the individual. That the visual framework does exert an effect in the judgments we admit in rejection of our second hypothesis. While the judgments were influenced in the direction of the tilted visual framework our data could in no way be interpreted to show that there was acceptance of the visual framework as the main determinant.

A dichotomous treatment of the postural and visual factors seems in no way to follow from the data of other investigators and certainly not from our own. The determination of the vertical is a joint function of the postural and visual factors and best judgments are rendered when the two sets of factors complement each other. In the present experiment the compromise was weighted in the direction of postural factors, but this might easily be an artifact of the experimental procedure and the equipment employed.

An interesting finding is that which was raised by the third hypothesis. When adjustments were made from the same quadrant as the tilted visual framework the larger constant error was found in each case when com-

pared with adjustments from the opposite quadrant. With regard to average error it was noted that when adjustments were made from the same quadrant the average error always equalled or exceeded that when the adjustments were made from opposite quadrants. It was noted when *S* made adjustments from the opposite quadrant he nearly always adjusted his body axis past the gravitational vertical and then returned himself in the direction of his initial tilt. When adjusting from the same quadrant the tendency was to stop short of the gravitational vertical and then approach it slowly usually not passing beyond it. The index of constant error as shown in Table I could be an end result of such a procedure. Under the former procedure adopted in returning from opposite quadrants the greatest use could have been made of the postural factors, as the force vector of the gravitational vertical could operate on both sides of the body axis giving a better basis for comparison and resulting in a smaller average error and a constant error not weighted as much in the direction of the visual framework. While the factors operating in this finding are not at this time clear, further work is indicated to explore in detail this finding.

The curves presented in Figs. 3 and 4 indicate a flattening effect at 15° and 20° of room tilt. It is true that only a few points were used in plotting and that room tilts of small magnitude were used. Plots of the individual data reveal similar trends and the curves presented here represent the data of each subject rather well. *Ss*' reports indicated that they were better able to recognize that the room was tilted when the larger tilts were used. They further reported that when they were sure the room was tilted they tried to adjust ignoring the visual framework. The data show definitely that they were unable to do this.

Summary

1. Five midshipmen from the Tulane Naval ROTC unit served as *Ss*. They were required to adjust a chair to the vertical positions from tilted positions in the lateral plane. The chair was located in a room which could be tilted to various inclined positions in the lateral plane. Adjustments were made in the presence of a visual frame of reference consisting of the entire room set 0°, 5°, 10°, 15°, and 20° from the gravitational vertical in the right and left quadrants. Ten determinations were made for each position of room tilt. *S* always began his adjustments from a tilted position of 45°.

2. The theory of Koffka that the accepted vertical will agree with the main lines of visual space though they may be in conflict with the postural cues was rejected under the conditions of this experiment.

3. Adjustments in the presence of a visual frame of reference showed equal variability when the visual cues presented were in conflict with the postural cues and when the two were in alignment.

4. When adjustments were made from tilted positions in the same quadrant as the tilted room the resulting average errors and constant errors in the direction of the tilted room were greater than when adjustments were made from tilted positions in the opposite quadrant from the tilted room.

5. With increasing amounts of tilt of the visual framework there was an increase in the size of average error of adjustment and of constant error in the direction of the tilted room. Statistical treatment revealed that only the differences between conflict in pos-

tural and visual cues and no conflict were significant.

6. Implications for the relative importance of postural and visual factors in the perception of the vertical were discussed.

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THE PERCEPTION OF OBSTACLES BY THE BLIND

BY PHILIP WORCHEL, JACK MAUNEY, AND JOHN G. ANDREW

University of Texas

Introduction

Since 1779 when Diderot (2) first recorded the 'amazing ability' of a blind friend to perceive objects and to judge accurately their distance from him, the avoidance of obstacles by the blind has been a source of wonder and speculation. Diderot attributed this ability to an increase in the sensitivity of the facial nerves and end organs.

Following Diderot's publication, numerous articles concerning this ability were reported and many theories were advanced in explanation of the phenomenon. Hayes (3), in his summary of this work, lists fourteen theories, which are divided into three groups: those based on a heightened response of some of the sense organs—the sensory theories; those which are based on a more acute interpretation of the normal responses of the sense organs—the perception theories; and the occult theories which explain the phenomenon in terms of magnetism, electricity, the subconscious, the development of a sixth sense, the vibration of the ether, or other hypothetical substance, or the action of vestigial organs of the skin.

In 1940 in the Cornell Psychological Laboratory, Supa, Cotzin, and Dallenbach (4) began their extensive studies to try to find the basis for the ability of the blind to perceive obstacles.

Four *S*s were used: two blind students who possessed the ability to perceive obstacles and two normally sighted graduate students of psychology. All of the *S*s served in seven series of experiments in which various sensory cues were eliminated.

On the basis of their results, the experimenters concluded that: (1) Stimulation of the skin by air waves *was not a necessary condition* for perception of obstacles. (2) Cutaneous surfaces were not sufficient for the perception. (3) *Aural cues formed the basis for obstacle perception.* It was impossible at this time for the authors to conclude whether or not the ability was due to pressure or auditory stimulation of the aural mechanism.

In search of the answer to this problem, Worchel and Dallenbach (5) began their experiment: "The determination of the sensory basis of the 'obstacle sense' by the isolation and separate investigation of the aural components." Deaf-blind *S*s in whom the auditory meatuses were intact but audition eliminated were used. Their results led to the conclusion that: (1) The cutaneous surfaces of the external ears (meatuses and tympanums) are not sufficient to the perception of obstacles. (2) The pressure theory of the obstacle sense is untenable. (3) Auditory stimulation is both a *necessary* and a *sufficient* condition for the perception of obstacles by the blind. *Therefore the aural mechanism involved in the perception of obstacles by the blind is audition.*

While Supa, Cotzin, Dallenbach, and Worchel were determining the source of the 'obstacle sense' in the blind, another group of scientists under the auspices of the Committee on Sensory Devices of the National Academy of Sciences was conducting extensive research on guidance devices for the blind at the Haskins Laboratories in New York City. This project was carried out during the period February 15, 1944 through December 31, 1947 under the direction of F. S. Cooper and Paul A. Zahl (1). The objective of the committee was the development of a portable device which would enable the average blind person to find his way about with more ease and efficiency than with the aid of the traditional cane or the guide dog. All of the devices developed were probe devices which were inherently limited to point-by-point exploration. The information which they presented to the blind was somewhat analogous to that which a sighted man would get by

viewing his environment through a soda straw. Eighteen different models were developed and subjected to tests at the Haskins Laboratories; however, only two of the devices performed sufficiently well to justify the hope that some instrument might eventually prove of general usefulness to the blind.

In order to determine the need and to evaluate the efficiency of any such device, it is necessary to assess the ability of the blind to orient themselves to their immediate environment without the use of a device. The results of the Cornell studies (4) and those of the Haskins Laboratories (1) show marked differences in the ability of the blind *S*s to perceive obstacles. Some of the blind *S*s could not perceive the obstacle at all, others collided with it frequently, and others reported perceiving the obstacle at various distances. Only four *S*s were used in each of the studies. Indoor conditions and various testing procedures were utilized, and the results treated differently. A distribution of the ability of the blind to perceive obstacles has as yet not been determined. The report of the Haskins Laboratories states, "'Obstacle sense' is often referred to in the literature of the blind, but little information is available as to the value of this special function or its distribution in the blind population." It is the purpose of the present study to determine the distribution of the ability of blind *S*s to perceive an obstacle under the usual outdoor conditions.

Procedure

The procedure duplicated that of the third experiment in the Cornell studies (4) of 1944 with the exception that the experiment was conducted outdoors. The obstacle was a ¼-in. masonite board 4 ft. wide and 4 ft. 10 in. high. This board was attached to a portable standard. Its lower edge was placed two ft. above the floor. Its upper edge, 6 ft. 10 in. above the floor, was therefore well above the elevation of *S*s' ears. This movable obstacle was placed 6, 12, 18, 24, 30, and 36 ft. away from the *S* on a concrete pavement 8 × 65 ft., bordered on both sides by grass. These distances were selected in a planned haphazard order, which guranteed that every position should be used equally often and without repeated sequence. Not only was the position of the board varied, but the subject was placed at different starting points in every trial. The starting positions, which were 0, 3, 6, 9, 12 ft. from the end of the walk, were varied in planned haphazard order also. After determining the starting position, *E* placed the obstacle 6, 12, 18, 24, 30, or 36 ft. from it. Sixty trials were given to each of the 34 totally blind *S*s. Six 'check' or false trials, that is, trials in which the obstacle was removed from the walk, were interspersed haphazardly in the last thirty tirals. At no time was the *S* brought up to the obstacle and shown his position after he had given his report. This experiment was not designed to study learning but merely to inform us of the ability of the blind subject to perceive an obstacle. Two reports were given by the *S*: (1) the point at which he first perceived the obstacle, *the 'first perception,'* and (2) the point of closest approach without touching the obstacle, *the 'final appraisal.'*

Instructions.—The following directions were read to each *S* before he began the experiment:

> A blindfold will be placed over your eyes so you will not have more skin showing than sighted people who also do this experiment. After you are blindfolded, you will be led around the experimental area in order that you will not know your location relative to the starting point or the obstacle. After a short time, you will be placed facing the obstacle near the right-hand side of the sidewalk. When you are tapped on the back, walk forward toward the obstacle. Walk near the right edge of the sidewalk and use this edge as a guide toward the obstacle. If you should leave the walk, immediately return to it and continue on toward the obstacle. When you first feel that something is in front of you, stop and raise your right arm. Then, lower your arm and continue to walk toward the obstacle. Approach it as closely as possible without touching it. When you have done that, raise your left arm and stand still. *Do not touch the obstacle.* As we wish to know how well you can do this, you will be asked to approach the obstacle many times.

Subjects.—Arrangements were made with the Superintendent of the Texas State School for the Blind, Austin, Texas, for all the 34 totally blind students at the School to serve in this study.[1] Total blindness was defined as 'no light perception.' All eye examinations were conducted by

[1] We are deeply indebted to Mr. W. E. Allen, superintendent of the Texas State School for the Blind, Austin, for his fullest cooperation in providing the subjects and facilities for the present study.

TABLE I

DISTRIBUTION OF THE MEAN FIRST PERCEPTIONS FOR THE 30 AND 36 FT. STARTING POINTS, MEAN FINAL APPRAISALS, AND NUMBER OF COLLISIONS FOR ALL 34 *Ss*

Mean First Perception (ft.)	Number of *Ss*	Mean Final Appraisal (ft.)	Number of *Ss*	Collisions	Number of *Ss*
27–29	1	5.0–5.4	1	16–17	2
24–26	3	4.5–4.9	0	14–15	0
21–23	2	4.0–4.4	0	12–13	0
18–20	2	3.5–3.9	1	10–11	0
15–17	1	3.0–3.4	1	8–9	1
12–14	3	2.5–2.9	1	6–7	1
9–11	8	2.0–2.4	1	4–5	3
6–8	9	1.5–1.9	1	2–3	7
3–5	5	1.0–1.4	3	0–1	20
		0.5–0.9	19		
		0.0–0.4	6		

local ophthalmologists. The ages of the students selected ranged from 8 to 23 years, the mean chronological age being at 14.8 years. Eighteen students were congenitally blind, the other sixteen became blind between the ages of 1 and 11 years. Our group included 16 females and 18 males. The *Ss* were chosen only on the basis of total blindness and not for any particular ability. They came from all parts of Texas, and include all but two totally blind *Ss* in the State.[2]

RESULTS

A summary of the findings on the mean 'first perceptions' for the 30 and 36 ft. points, mean final appraisals, and number of collisions for all the 34 *Ss* appears in Table I. The 'first perceptions' of the 30 and 36 ft. positions were selected since many *Ss* perceived the board immediately upon being led to the shorter starting positions at 6, 12, 18, and 24 ft.

As the earlier studies show (4, 5), no one criterion is sufficient to demonstrate the ability of the blind to perceive obstacles. The number of collisions, consistency in 'final appraisals,' the number of 'false' perceptions on the check trials, the relationship between the distances of the reported perceptions and the starting points, all have to be considered in the evaluation of the ability of the blind to perceive obstacles. *Ss* who collide with the board more frequently than three times out of 60 trials, and report 'false' perceptions have little or no ability to perceive obstacles as demonstrated by previous experiments (4, 5). *Ss* whose 'final' appraisals and 'first perceptions' increase with an increase in the starting distances do not possess the 'obstacle sense.' Their reports are due to chance and not to any perceptual ability (5).

First perceptions.—An analysis of the mean 'first perceptions' for every starting point reveals three significant trends. In some cases, the average at one starting point is representative of the averages at every other point, that is, consistency in 'first perceptions' from the various starting points. Thus, *DE*'s averages at the different starting points are 2.78 at 6 ft.; 3.33 at 12 ft.; 3.00 at 18 ft.; 3.00 at 24 ft.; 3.44 at 30 ft.; and 2.78 at 36 ft. For other *Ss*, we find an increase in first perception with an increase in the distance of the starting points, i.e., the

[2] For tables containing additional details concerning the subjects and the experimental results order Document 2726 from American Documentation Institute, 1719 N St., N.W., Washington 6, D. C., remitting $0.50 for microfilm (images one in. high on standard 35 mm. motion picture film) or $1.40 for photocopies (6 × 8 in.) readable without optical aid.

further the starting-distance the larger the averages. This increase, however, is demonstrated in two ways. Some *Ss* reported perceiving the board immediately upon being led to the 6, 12, 18, and 24 ft. starting points. Their increase in 'first perception' with increase in starting-distance is due to the fact that their true ability is greater than that measured by the smaller starting-distances. For example, *JF*'s averages are: 6.00 at 6 ft.; 11.77 at 12 ft.; 16.55 at 18 ft.; 21.88 at 24 ft.; 22.88 at 30 ft.; and 23.44 at 36 ft.

The increase of 'first perception' with starting-distance for other *Ss* could not be explained on this basis. *JB*'s averages were: 1.36 at 6 ft.; 2.94 at 12 ft.; 5.50 at 18 ft.; 7.78 at 24 ft.; 8.67 at 30 ft.; and 9.67 at 36 ft. These results stand in striking contrast to those of the former *Ss* and they indicate that the determining factor in these *Ss*' reports was not their perception of the wall but the distance of the starting points from the wall. Sheer chance within the limitations set by the distances of the starting points will account, as we believe, for these results. The chance hypothesis is corroborated by the fact that *JB* gave six 'false perceptions' on the six check trials. He ran into the obstacle five times, and three of these five collisions occurred prior to the 'first perception.' In addition, his mean 'final appraisals' increased with an increased in the starting-distances.

Table I shows that the mean 'first perceptions' of the 30 and 36 ft. starting points range all the way from 2.5 ft. to 29.5 ft. The median of this distribution is 9.6 ft.

'*Final appraisals.*'—The outdoor conditions of the present experiment presented many distracting cues. The concrete walk used in the experiment was only 50 ft. from one of the main streets in Austin. Traffic noise and overhead airplanes were particularly disturbing. Changes in wind direction, sunshine and shade with the attendant temperature changes had to be interpreted properly. Despite these distractions, the general averages of our *Ss*' attempts to approach the wall "as closely as possible without touching it" are small. Twenty-five *Ss* gave mean 'final appraisals' of less than 1.0 ft. (Table I). Much smaller individual differences are observed in the 'final appraisals' than in the 'first perceptions.' The ability to first perceive the obstacle differs greatly from one blind *S* to another; but all the *Ss* who possess the 'obstacle sense' know when they are almost 'on top of it.'

The fractionation of the data and the separate computation of the averages for every single starting point reveal that for *JB*, *JD*, *JM*, *DP* the 'final appraisals' increase with the distance at which our *Ss* started walking toward the wall. For example, *DP*'s 'final appraisals' average 1.56 at 6 ft.; 2.50 at 12 ft.; 4.17 at 18 ft.; 3.56 at 24 ft.; 4.17 at 30 ft.; and 5.56 at 36 ft. These results are in opposition to those of the other 29 blind *Ss* in which the 'final appraisals' averaged approximately the same for all the starting points. For example, *DC*'s 'final appraisals' averaged .67 at 6 ft.; .64 at 12 ft.; .56 at 18 ft.; .64 at 24 ft.; .44 at 30 ft.; .56 at 36 ft. Thus, for the four *Ss* mentioned above, the results indicate that distance, and not the perception of the wall is the factor determining our *Ss*' 'final appraisals.' Since the greater distances would by chance yield larger 'final appraisals,' the chance hypothesis seems to be the most reasonable explanation of our results. We are led to believe that these *Ss* do not possess the ability of perceiving obstacles. This conclusion is supported by the number of false

perceptions reported by these *S*s, by the number of collisions, and by the large mean variation in mean 'first perceptions' and 'final appraisals.'

Vexierfehler.—Six 'blank' trials were conducted, as will be recalled, during the last 30 trials. *JB*, *JD*, and *BW* reported 'first perceptions' and 'final appraisals' in all six trials. *GK* reported 'first perceptions' and 'final appraisals' in five 'blank' trials. *DP* and *MS* gave three false 'first perceptions'; *JC* and *JM* gave two; *SJ* and *SL* gave one; and all the others (24) made none.

Since the *S*s did not know when the 'blank' trials occurred, or even that they were to be given, their reports in these trials may be samples of their reports in the entire experiment. In *RH*'s case it appears from casual inspection that they are. In six 'blank' trials he did not report a single 'false perception.' In 54 trials with the obstacle, he collided with it 16 times. Though the results of the 'blank' trials, considered alone, might suggest that *RH* possessed the ability to perceive obstacles to a high degree since he made no 'final appraisals,' i.e., did not report the obstacle when it was not present, that is not, as we believe, the explanation. The reason that he did not report the obstacle in the 'blank' trials when it was not present was because he did not report it in the 'main' trials when it was present. Not to report was the pattern of his responses. He was behaving the same in the 'blank' trials as in the 'main' trials.

On the basis of the results on the 'blank' trials, we may conclude then that *JB*, *JD*, *BW*, *GK*, and *RH* do not possess the ability to perceive obstacles. The occasional 'false' reports made by some of the other *S*s may be due to distracting cues, such as change in temperature, wind, or sudden noises.

Collisions.—The collisions were divided into two types: (1) those which occurred before the *S* perceived the obstacle; and (2) those which followed the signal of perception.

Invariably the first type was a jolting collision in which the *S* crashed forcefully into the obstacle without any apparent premonition that it was before him. *TC* and *SJ* collided with the obstacle in this manner. *SJ* suffered a collision of this type on her first attempt and *TC* collided before perception on his third approach. As these *S*s later performed with sufficient ability to justify the belief that they do possess the obstacle sense, these errors probably resulted from nervousness due to the testing conditions.

JB and *RH* also collided with the barrier in this manner. Their collisions were not localized near the first few trials but were dispersed throughout the experiment. In addition to this fact, *RH* had 16 collisions. Although *JB* only collided with the barrier 5 times, she gave a false perception on every single blank trial.

The second type of collision occurred after the *S* had signaled that he knew the barrier was in front of him. Many of these collisions were gentle contacts which frequently resulted from attempts to improve the final appraisal. The consistency and closeness of approach of *HG* indicates that he was more acute than any other *S* on his final appraisal. Yet he had four collisions of this type.

Only *JB*, *JD*, *RH*, *GK*, and *JM* crashed heavily into the barrier after giving the signal for initial perception. They continued to walk until they suffered a jolting collision. In view of this erratic behavior, inconsistency of performance, and their complete lack of confidence, it seems that these

Ss do not possess the ability to perceive obstacles.

Ss' comments.—Without exception, the experience of perceiving the barrier was explained as a general bodily sensation which felt very similar to a shadow falling upon the *S* or as if an obstruction had blocked off the wind. Only three *Ss* indicated that they were even aware that obstacle perception was a function of audition. *WP* said he had once read that audition was of primary importance in perceiving obstacles. *TC* and *JB* each remarked spontaneously during the experiment that they did not hear the obstacle so well. With both *TC* and *JB* this spontaneous remark occurred only once during the experiment and on both occasions it followed a particularly poor performance. Following the conclusion of their 60 trials, neither of these *Ss* mentioned audition in their explanation of their ability.

Summary and Conclusions

The present study was undertaken to determine: (1) the distribution of the ability to perceive obstacles among 34 totally blind *Ss*, (2) to determine the need for an obstacle avoidance device, and (3) to serve as a basis for evaluating the efficiency of an obstacle avoidance device.

A movable obstacle was placed at pre-determined positions on an outdoor concrete walk. The *S* was placed in a planned, haphazard manner 6, 12, 18, 24, 30, or 36 ft. from the obstacle in 54 trials. Six 'blank' trials were interspersed during the latter half of the series. The *S* was instructed to report as soon as he perceived the obstacle (first perception), and then to stop when he approached it as closely as possible without touching it (final appraisal). The results indicate:

1. Large individual differences in the ability to first perceive the obstacle.

2. Consistency in the 'final appraisals' of those *Ss* who showed evidence of possessing the 'obstacle sense.'

3. Seven of the 34 *Ss* (21 percent) did not possess the 'obstacle sense' as determined by the number of collisions, the number of false perceptions, the size of the mean variation of the mean 'first perception' and 'final appraisal,' and correlation of mean 'first perception' and 'final appraisal' with starting-distance.

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MIXED DISTRIBUTION OF PRACTICE IN MIRROR DRAWING[1]

BY J. C. TSAO

Hong Kong University

Introduction

In an experiment on mirror drawing, in which two-min. intervals were interpolated between pairs of trials, Snoddy (4) found that the efficiency of the second trial in each pair was definitely lower than that of the first one. The author considered the loss in the second trial to be due to the interference between primary and secondary growths which, in his words, "were distinctly opposite in character."

Under the conditions in his experiment, Snoddy thought that "we should have primary growth in the first circuit and secondary growth in the second, since continuity is a determiner of secondary growth, but both should be under the dominance of the preceding time interval." In other words, the time interval before the paired repetitions should enhance primary growth which was a positive function of the time elapsing between practice periods, and in the first trial the primary growth could develop normally; but in the second repetition, secondary growth occurred because of the continuity of trials, and thus impaired both growths and caused loss.

In Snoddy's results, however, the evidence was not conclusive. In his experiment there were two pairs of trials preceded each by a 24-hour interval. There was no loss in the second trial of the pair after the first 24-hour recess, and there was a loss after the second over-night interval. Though Snoddy considered the outcome after the second long interval as "a case following a long time interval, when the initial circuit caught the loss coming from this long interval, which certainly eliminates it as a true exception to the principle," it was rather dubious whether there should be losses in the second trials of paired repetitions preceded by longer intervals. Further experimental studies were needed.

Snoddy's theory of primary and secondary growths has aroused much interest in the psychological world. Many investigators, Bell (1) and Doré and Hilgard (2, 3), to mention only a few, have carried out critical or experimental studies of it. All of these studies concerned, however, the effects of early and late, or longer and shorter, intervals, or improvements during or between practice; none of them, as far as the present writer knows, was about the nature of, and the relation between, the two growths assumed by Snoddy.

Interferences represent, on the one hand, a relationship between the two growths, and may show the nature of them on the other hand. A study of the interferences, under the condition of mixed distribution of practice, would, therefore, throw some light on the nature of primary and secondary growths, and lay a basis for the evaluation of Snoddy's theory which purported to explain certain phenomena that are generally observed in spaced and massed learning. It was for this purpose that the present study was carried out.

Experiment and Results

The experiment in the present study was a part of the experiment on mirror drawing reported in the first article of this series (6). A brief recapitulation and some supplements should suffice. The apparatus used was shown in the former paper. In general, it was slightly dif-

[1] This is the sixth article in a series of studies in spaced and massed learning, the other five having been published or sent to press (6–10). This study was carried out in the Psychological Laboratory, Cambridge, England; the writer is indebted to Mr. G. C. Grindley for help and advice.

ferent from the ordinary star-pattern tracing sheet. The learning score was $\frac{1}{10}$ of the actual time, in terms of sec., taken by the *S* in each trial of tracing, which corresponded to the mean time required in tracing from the centre of the apparatus to one of the peripheral circles and back to the centre.

The *S*s were 16 R.A.F. staff; they formed the Group C which appeared in the former paper. Each *S* practised 12 trials in six sittings with two continuous trials in each session; in each day there were two sittings, one in the morning and one in the afternoon.

The average scores were presented in the former paper and are reproduced below:

Trial:	1	2	3	4	5	6	7	8	9	10	11	12
Score:	16.0	11.1	8.6	7.1	5.6	5.2	4.6	4.4	3.9	3.5	3.3	3.0

Since the scores are numbers of sec. in time and consequently denote the reciprocals of the speed of tracing, a larger figure means a lower efficiency.

The learning scores show clearly that the second trials in paired repetitions had no decrement but increment in efficiency. The differences in scores between continuous trials and between successive trials separated by intervals are as follows:

Trials:	1,2		3,4		5,6		7,8		9,10		11,12	Av.
Diff. between cont. trials:	4.9		1.5		0.4		0.2		0.4		0.3	1.28
Diff. between sep. trials:		2.5		1.5		0.6		0.5		0.2		1.06

On the average the decrease in scores or increase in efficiency from one trial to another in continuous repetitions is slightly higher than that from one trial to another in trials separated by intervals, but the difference is small. The increments in achievement between separated trials are more regular, though the number of trials is only 12, too small to be seriously treated statistically. This may indicate one of the characteristics in the general trend of increments in time-interval and in continuous practice, that is, there are usually more fluctuations in massed than in spaced trials.

Discussion

The results of the present study were quite different from Snoddy's. The conditions in the present experiment were, however, also different from those in Snoddy's experiment. We should first, therefore, make sure whether the differences in results could be explained by the differences in experimental situations. The lengths of the intervals used in the two studies were different, but this should not cause discrepancies in the results, since in Snoddy's experiment both two-minute and 24-hour intervals were used and there was no reason why half-a-day's intervals should give rise to different outcomes.

There was one difference in the experimental procedures of the two studies to which Snoddy would, perhaps, attach much significance. In Snoddy's experiment the first two trials were not paired. According to him, the loss in the second trials of paired repetitions "does not occur if the pairing is begun at the first repetition" (5). In the present study the pairing did begin at the first trial. It would seem that our results confirmed Snoddy's supposition, but that was actually far from being the case.

Snoddy contended that the loss in the second of paired trials "does not occur if the pairing is begun at the first repetition, or if we have five, six, or any number of repetitions in continuous sequence from the beginning of practice," and he cited the results of his Experiment 2 as evidence. In his experiment trials were separated by half-minute intervals. A scrutiny of his data (4, p. 27, Fig. 3) revealed that 20 out of the 57 trials showed decrements in comparison with the immediately preceding ones. These data did not fit in with Snoddy's contention quite well. What his data showed are obviously fluctuations, rather rhythmic fluctuations, in learning efficiency. These fluctuations may be due to, among other factors, the particular experimental technique used by Snoddy. There were niches on the path of his apparatus, which made the tracing much more difficult than it is on an ordinary mirror tracing sheet or on the apparatus used in our experiment. Moreover, 'pacing' was applied in his experiment; "the *S* is paced so that the number of errors and the number of seconds required for a circuit of the instrument are statistically equal." With such a requirement, one error in average made in one sec., not more and not less, and such an apparatus with niches on the path, the mirror drawing in Snoddy's experiment was not an easy task. Fluctuations in learning were natural, and it is not without justification to imagine that the second trials in paired repetitions were in a specially disadvantageous position.

When Snoddy maintained that the loss in the second of paired trials "does not occur if the pairing is begun at the first repetition," he was attributing the interferences which caused the loss to the experimental procedure in which the *S* was "first adapted to time-interval conditions and then shifted to continuous practice conditions *in the same practice sitting*." In other words, the loss was due to the disturbance in the adaptation of the *S* to practice situation because of the shifting from one condition to another. We remember, however, that formerly he attributed the loss to the dominance of the time intervals preceding paired trials.

Snoddy must face a dilemma: If the loss was due to the dominance of the preceding time interval, it should not matter whether the first two trials were paired or not, since at any rate succeeding pairs would be preceded each by an interval; if the interferences were due to the experimental procedure in which the *S* was "first adapted to time-interval conditions and then shifted to continuous practice conditions *in the same practice sitting*," there should be no loss after an over-night interval, since separated by 24 hours the pairs of trials could not be in the same practice sitting.

In short, the results of the present study did not confirm Snoddy's findings; his results might be an outcome of his particular experimental technique and his hypothesis could not well explain them. This would inevitably render Snoddy's theory of mental growth questionable, at least as far as the interference or 'opposedness' between the two growths is concerned.

Summary and Conclusion

Sixteen *S*s practised on mirror drawing in two continuous trials at each sitting, and they performed altogether 12 trials in six sittings during a period of three days. The learning progressed from trial to trial and did not show any regression at the second trials of paired tracings. The results did not substantiate Snoddy's theory

of mental growth and thus rendered it questionable, at least as far as the interference or opposedness between the two growths is concerned.

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AN EVALUATION OF EXPERIMENTAL METHODS OF TIME JUDGMENT

BY JOHS CLAUSEN

New York Psychiatric Institute, Columbia University

A common symptom in psychotic patients is a disturbance of what is known as 'time sense.' This symptom presents itself either as a lack of orientation in time, or as the subjective impression that time is passing unusually slow or unusually fast, as the case may be.

Many problems are hidden behind the term 'time sense,' most of which have been unexplored. One problem that has been investigated in normals, and to a certain extent in schizophrenic patients, is the ability to judge short intervals of time. Even this type of investigation is somewhat clouded by differences in experimental procedure and lack of uniformity in terminology. A discussion of terminology and the relative merit of the different methods can therefore be of interest for students of normal individuals and psychotic patients.

The methods used by investigators of the judgment of short time intervals can be listed as follows: (1) Presentation of an interval by the examiner which the subject is asked to reproduce (usually referred to as the Method of Reproduction). (2) Verbal estimation by the subject of an interval set by the examiner (usually referred to as the Method of Estimation). (3) Demonstration by the subject of an interval stated by the examiner (usually referred to as the Method of Production). (4) A fourth method, one of comparison, has sometimes been employed where two intervals are presented and the subject is asked to indicate which is longer, but this method has not been used in the more recent studies.

Even the names of the methods present problems. There is no objection to calling the first method the Method of Reproduction, since it is descriptive of the actual task performed. The term 'estimation,' however, could just as well be used for method (3) as for method (2). We have preferred to call method (2) Verbal Estimation and method (3) Operative Estimation. These names are more descriptive, the common element of estimation being emphasized, and the term 'production' of time avoided.

The three methods, Reproduction, Verbal Estimation and Operative Estimation, have a common element in that they have a stimulus interval, presented by the examiner whether it is named in seconds or demonstrated by an operation, and a response interval, given by the subject which can also be named or demonstrated. It is, of course, the relation between the experimenter's stimulus interval and the subject's response interval that is of interest. This relationship has usually been stated in terms of overestimation and underestimation. This has led to some confusion since the stimulus as well as the response interval can be used as a reference point. In some cases overestimation has been used when the response interval was longer than the stimulus interval, in other cases when the stimulus interval was longer. We have found it more reasonable to take the stimulus interval as our standard reference point and use the term underestimation when response time is shorter than this standard, and overestimation when the response interval is longer than this standard.

The relative value of the different methods has not come in for thorough discussion in the literature. Spencer (13) and Yerkes and Urban (15) have criticized the Method of Verbal Estimation because of the predominance of estimates ending with the preferred digits of zero and five. Spencer also found that with the Method of Reproduction more

accurate results were obtained than by statements in terms of time units (Verbal Estimation). Gilliland and Humphreys (9) found that Reproduction is perhaps easier than Verbal and Operative Estimation, but these all seem to be closely related mental processes, according to them. In an unpublished study Johnston (12) combined the results from six previous studies and concluded that the methods of Verbal and Operative Estimation are equivalent since results obtained by these methods are the same.

One of the most fundamental observations with regard to time judgment is that during an interval filled with interesting activity time seems to move rapidly and during intervals filled with dull activity time seems to move slowly. The kind of filling, and the attention paid by the subject to the filling element have been found to be of importance.[1] As Gilliland, Hofeldt, and Eckstrand (8) point out, however, it is difficult to distinguish between filled and unfilled intervals. Visual, auditory and other external stimuli used to fill an interval may have little attention value for the subject. On the other hand, the subject's thoughts, memories or phantasies during the unfilled interval may constitute important filling.

Another basic problem with regard to time judgment is the significance of the duration of the intervals. Will short time intervals be judged differently than longer ones?

Woodrow (15) worked with intervals from .2 to 30.0 sec. and found that each of the intervals was underestimated by some subjects and overestimated by others. Johnston (12), using the Method of Operative Estimation, compared normal subjects with schizophrenic patients. She found that normals judged 5- and 10-sec. intervals accurately and underestimated intervals of 30 sec. and greater, whereas the schizophrenics overestimated 5 and 10 sec., and underestimated the longer intervals.

[1] For reference to the literature of this problem see Gilliland, Hofeldt, and Eckstrand's review (8).

In a recent investigation (the Columbia Greystone Project) concerned with the therapeutic effect of frontal lobe operation on mental patients, 43 schizophrenic patients were tested for their ability to judge short time intervals. Nineteen of these patients had various parts of their frontal lobes removed and the remaining 24 patients served as a control group. All these patients were tested at specified intervals, before and after the brain operations were performed. Since the three methods of Reproduction, Verbal Estimation, and Operative Estimation were utilized, the data obtained may be analyzed to evaluate the adequacy of the different methods in the judgment of time. The operations were not found to alter in any way the function of time judgment in these patients (Clausen, 2), and therefore the operated and control patients have been treated as one group in this report. The original preoperative testing will be referred to as the O test. The repeated testing 4½ months later will be referred to as the R test.

The Experiment

The apparatus used in this experiment was an electrically driven stop clock attached in series with a very dim lamp and a key. When the key was depressed the clock started and the lamp lighted, when the key was released the clock stopped and the light extinguished. The lamp could be replaced with a buzzer in the circuit by simply turning a switch. This arrangement made it possible to record judgment of time for filled (buzzer) and unfilled intervals (lamp).

For the method of Reproduction the experimenter pressed the key for 5, 10, and 15 sec. in a random sequence. Placed so that he could not see the face of the clock, the subject had to reproduce the same time interval immediately. For each interval an equal number of trials were given with the lamp (unfilled intervals) and with the buzzer (filled intervals).

For the method of Verbal Estimation the experimenter pressed the key for a given interval, and the subject was asked to estimate the duration of the interval.

For the method of Operative Estimation the subject was asked to press the key for an interval stated by the experimenter.

The intervals utilized for all three methods were of 5, 10, and 15 sec. duration. Each interval for each method had an equal number of

filled (F) and unfilled (U) trials given in random order. The experiment thus consisted of 18 subtests.

Results

Performance on the filled intervals was compared with performance on the unfilled for the three different intervals utilized for the three different methods. This was done for the O and R test separately. When the method of critical ratios was applied, none of the comparisons reached the .01 level of confidence. We can therefore conclude that this type of filling does not influence our subject's ability to judge time intervals.

The means for the three different intervals obtained by the three different methods separately for the original and the repeated test are presented in Table I. In accordance with the finding of no significant differences between the filled and unfilled intervals they are combined in this table. At the top of the table is given the duration of the stimulus interval.

The table shows that the Method of Verbal Estimation consistently results in a marked overestimation. The Methods of Reproduction and Operative Estimation have a tendency to result in underestimation for the 10- and 15-sec. intervals and overestimation for the 5-sec. intervals, although this tendency does not become apparent until the R test for the Method of Operative Estimation.

By the Method of Verbal Estimation decidedly less accuracy in the judgments was obtained than by the other two methods. The R test by this method gives means that are closer to the stimulus intervals than is the case for the O test, but they are still overestimated approximately 100 percent. The difference in the means from the O test to the R test for Reproduction and Operative Estimation is very slight, and what changes occur do not consistently go in the direction of increased accuracy.

The intercorrelations between the subtests for the three methods have been computed. For each method the three intervals are treated separately for unfilled and filled condition, and separately for the O and the R test.

The data obtained by the Method of Reproduction show marked scattering of the correlation coefficients for different combinations of subtests. The coefficients vary for the O test from +.19 to +.93 and for the R test from −.14 to +.76. There is also marked difference in the correlation between the O and the R tests for the same combination of subtests. In one case the correlation for the O test is +.93 and for the R test −.14.

The Method of Verbal Estimation has a general tendency to higher and more uniform correlations between the subtests. The range is from +.70 to +.92 for the O test and from +.77 to +.99 for the R test. The difference in the correlation coefficient between the O and the R testing intervals for

TABLE I

The Means for the Three Intervals Obtained by the Three Different Methods

	Reproduction			Verbal Estimation			Operative Estimation		
	5 sec.	10 sec.	15 sec.	5 sec.	10 sec.	15 sec.	5 sec.	10 sec.	15 sec.
O	5.60	9.87	13.68	14.49	27.37	33.82	4.42	8.40	11.55
R	5.19	9.41	13.42	10.03	20.52	29.60	6.53	10.70	12.99

the same combination of subtests is rather small.

The Method of Operative Estimation gives correlation coefficients between the subtests very similar to those for Verbal Estimation. They are rather high and uniform with a range of +.60 to +.92 for the O test and +.78 to +.97 for the R test. The difference from the O to the R test in any one combination of the subtests is also rather small here.

The lack of reliability in the data obtained by the Method of Reproduction is also apparent from the intercorrelations of totalled scores shown in Table II. In this table the correlations between the data secured with the different methods are presented. For each method the scores from all

TABLE II

INTERCORRELATIONS OF THE TOTALLED SCORES FOR THE THREE DIFFERENT METHODS

		Verbal Estimation	Operative Estimation
Reproduction	O	−.05	.04
	R	−.55	.36
Verbal Estimation	O		−.63
	R		−.42

three intervals, both filled and unfilled, are totalled. This total score is correlated for the O and the R test separately for each method.

Where Reproduction is involved there is a striking difference between correlations on the O and on the R test. The correlations between the two forms of Estimation are much more consistent, higher and negative. The tendency is toward negative correlation between Reproduction and Verbal Estimation, and toward positive correlation between Reproduction and Operative Estimation.

DISCUSSION

It seems evident from the results that the task of Reproduction involves a different underlying function than do Verbal and Operative Estimation, since the intercorrelations between the subtests have a much wider range, and since the subtests show markedly less reliability than is the case for the two other methods. The Reproduction task is independent of any relation of personal time to objective time. If an interval of 15 sec. is presented to a subject, he may be able to reproduce it fairly accurately regardless of what his verbal estimation of the interval would be. It could be called 5 sec. or 50 sec., but the reproduction would still be accurate as long as he is consistent in his judgment from demonstration to reproduction. The Method of Reproduction produces average judgments that are closer to the stimulus interval than is the case in either of the other two methods. In spite of this fact, Reproduction shows more instability in the intercorrelations between the subtests than do the other two methods. This indicates that Reproduction measures a function of less uniform character than do Verbal or Operative Estimation, and for this reason the latter methods are to be preferred to the Method of Reproduction.

It is difficult to understand this difference between Reproduction and the other two methods. It might be that attention is more easily interfered with if the subject has to concentrate first on the demonstration and then on the reproduction, as is the case in the Reproduction Method, whereas Verbal Estimation and Operative Estimation require attention only for the stimulus interval *or* the response interval.

In contrast to Reproduction, the Methods of Verbal and Operative Estimation deal with the relation of personal time to clock time. If his personal time moves rapidly, he would by Verbal Estimation name a demon-

strated interval to be longer than its clock value, but by Operative Estimation keep the key down for a shorter interval then required. This difference between the two methods is indicated by the negative correlation between the results for the tasks. We do not know, however, how justified we are in placing emphasis on personal time, whether there is a general tendency in a person to experience passage of time as 'slow' or 'fast.'

The intercorrelations of the subtests for the two methods of Estimation show that the functions measured are relatively circumscribed and are independent of the length of the stimulus interval. The correlation between the two tasks is sufficiently high to indicate an inverse relation between the two, and we may be satisfied with data from one method in future experimentation. The Method of Operative Estimation is to be preferred, since it gives somewhat more accurate judgments with less scattering of scores, and eliminates the tendency toward verbal rounding of interval judged.

Our finding that Reproduction is measuring a different underlying function than the two methods of Estimation explains the confusion that has existed on how to compare the results obtained by the different methods. Reproduction yields results that are not interchangeable with results from the other methods.

With regard to the duration of the stimulus intervals our results are in general agreement with previous studies, namely that shorter intervals have a tendency to be overestimated and longer ones to be underestimated. The results obtained by Verbal Estimations, however, do not follow this rule, as here all intervals are overestimated. In keeping with Woodrow's (15) finding we observed that each interval for Reproduction and Operative Estimation was underestimated by some of the subjects and overestimated by others. In the group, however, there is a distinct tendency to overestimate short intervals and underestimate the longer intervals.

The neutral point between overestimation and underestimation in our data lies between 5 and 10 sec. In Hoering and Vierordt's finding it was below one sec., and in Johnston's (12) study of normal subjects and schizophrenic patients it seems to be somewhere between 10 and 30 sec. No explanations present themselves for this lack of agreement.

Since the data on which we have based our conclusions have been collected from schizophrenic patients, it remains to be seen whether they also would be valid for normal subjects. The final answer can of course only be given by analysis of data obtained from normals. It seems reasonable, however, to believe that the fundamental difference between Reproduction on one side, and Verbal and Operative Estimation on the other, also would be found in normals. It might be found, however, that the function measured by Reproduction has a more uniform character in normals where attention to the task is more easily obtained. It also seems justified to believe that the two forms of Estimation would correlate negatively in normals as well, since they seem to be opposite aspects of the relation between personal time and clock time. The overestimation of shorter intervals and underestimation of longer intervals have been clearly demonstrated in normals by other investigators.

Future experimental investigations of the ability to judge short time intervals would probably be best conducted in normals also by using the Method of Operative Estimation. This would most certainly be so if one wants to obtain data with which the performance of schizophrenics may be compared. If the problem is to obtain information about normal subjects' time-judging ability *per se*, one should avoid using the Method of

Reproduction until it has been demonstrated that the function measured by this method is of a more uniform character in normals than in our schizophrenic patients. Even if this should be demonstrated, one should remember that the results obtained by Reproduction and Operative Estimation are not interchangeable.

Summary

An attempt has been made to evaluate three different experimental methods of measuring the ability to judge short time intervals. A group of 43 schizophrenic patients were tested and retested utilizing the methods of Reproduction, Verbal Estimation and Operative Estimation. The intervals were of 5, 10, and 15 sec. duration, and they were presented both as filled and unfilled intervals. Some problems of terminological nature were discussed and the following conclusions were arrived at:

1. The judgments were not altered significantly for any of the three methods, regardless of whether the intervals were filled with the sound of a buzzer.

2. By the Method of Verbal Estimation the intervals were consistently overestimated.

3. By the methods of Reproduction and Operative Estimation the general tendency is to overestimate the 5-sec. interval and underestimate the 10- and 15-sec. intervals.

4. The results obtained by the Method of Reproduction are less consistent and less reliable than those obtained by the two other methods, and seem to stem from a different underlying function.

5. The methods of Verbal Estimation and Operative Estimation can be regarded as measuring the same function inversely, since they have a high negative correlation. Of the three methods Operative Estimation is to be preferred.

6. There is reason to believe for normal subjects also the Method of Operative Estimation is to be preferred.

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A FURTHER TEST OF THE ABILITY OF RATS TO LEARN THE LOCATION OF FOOD WHEN MOTIVATED BY THIRST

BY HOWARD H. KENDLER AND JOSEPH H. KANNER

New York University

At the 1949 meeting of the Eastern Psychological Association, Professor Robert S. Woodworth suggested one possible reason why Kendler and Mencher (2) and Grice (1) failed to obtain any evidence of rats learning the location of an unwanted goal object.[1]

These experimenters had trained *thirsty* animals in a simple T-maze having *food* and water in one end-box and only water in the other end-box. Following a training series, during which time the *S*s had an equal number of runs to both sides of the maze, the motivation of the *S*s was shifted to hunger. In neither experiment did the *S*s provide any evidence of having learned the location of the food, i.e., they responded in a chance manner on the first test trial. These results were interpreted as being inconsistent with the perceptual non-reinforcement position of Leeper (3) and Tolman (4).

Professor Woodworth suggested that these investigators (1, 2) may have tried 'too hard' to have their *S*s form a perception of the location of food. Reasoning within his own theoretical framework (essentially a perceptual-reinforcement theory), he postulated that perceptual responses, like conditioned glandular and motor responses, are capable of experimental extinction. He hypothesized that what had occurred in these experiments was that the animals had formed or 'registered' a perception of the location of food when thirsty; but because this perception was never followed by *eating*, it became extinguished. In Grice's experiment the training series consisted of four trials daily for 12 days; in Kendler and Mencher's experiment the *S*s had four trials daily for seven days. Professor Woodworth reasoned that if the food was not available until the end of the training series, then the perception of the location of the food, which would be formed following the sudden introduction of food, would not have sufficient time to become extinguished. One would expect, therefore, that if food was introduced into one of the goal boxes during the terminal part, rather than at the beginning, of the training series, the *S*s would choose the side leading to food when hungry.

The present investigation is a test of Professor Woodworth's hypothesis.

Experimental Procedure

The experimental procedure employed in this study is essentially the same as that used by Kendler and Mencher (2), except for the modifications required to test Woodworth's hypothesis. Since the procedure is described in detail elsewhere (2), only a brief description is presented here.

The apparatus was a single-choice T-maze. In this study the right half was painted black while the left half was painted gray. In Kendler and Mencher's maze the right half was painted white and a decided preference for the left (black) side was exhibited. Although the black-gray distinction used in this experiment reduced the degree of the preference, it definitely did not eliminate it. In each end-box was a sheet-metal 'table' which was placed against the wall farthest from the choice-point. The table top contained five circular holes which permitted the insertion of five jigger glasses.

The *S*s for this experiment were 31 naive male albino rats (approximately 80 days old) of the Wistar strain, purchased from the Albino Farms, Red Bank, New Jersey.

[1] Professor Woodworth's comments were made during a discussion period following an experimental report at the 1949 meeting of the Eastern Psychological Association.

Following a two-day preliminary training period the training series was initiated. The training series consisted of four trials daily for seven days. During this time the *S*s were motivated by a thirst drive based upon approximately 22½ hours of water deprivation and satiated for food. In both goal boxes *one* of the five jigger glasses contained water. The position of this glass was varied from trial to trial. Thus the thirsty *S* in his search for water would be forced to '*perceive discriminatingly*' the contents of the glasses in each goal box.

During the first six days of the training series the four remaining glasses in *both* goal boxes remained empty for Group 1. On the seventh day, food (Purina dog checkers) was placed in four of the glasses on one side, while the fifth glass still contained water. The four remaining glasses in the other goal box remained empty. Thus for Group 1 the animals would suddenly see the food on the last day of their training trials. The procedure for Group 2 was essentially similar except food was placed in the four glasses on one side during the sixth and seventh day of the training series. Group 1, therefore, had one day in which to form a cognition of the location of food while Group 2 had two days.

Half of the animals in each group had equal experience in both end-boxes. This was accomplished by having the initial run of each day a free choice: the second a forced trial to the side opposite that chosen on trial one; the third a free choice; while the fourth was a forced trial to the side opposite that chosen on trial three. At least 15 min. elapsed between successive runs of the same animal. This procedure was similar to that used by the previous investigators (1, 2) to equate the experience of their animals with the content of both goal boxes.

Perhaps the use of the forced trials in some manner interfered with the development of adequate cognitions. In order to test for this possibility, the remaining half of each group had free choices on all of their trials. Massed practice was used to induce as much spontaneous alternation as possible.

The rats with a 'free, forced, free, forced' routine will be designated by F while the animals subjected to mass practice will be designated by M. The four experimental groups, therefore, are as follows: Groups 1F, 1M, 2F, 2M. The number refers to the number of days food was present in the maze, while the letter refers to the manner in which the trials were given.

The *S*s were allowed to drink the water for five sec. in either goal box. Unlike the technique used by Kendler and Mencher, no food was placed, at any time, behind either of the goal boxes.

The test series consisted of four daily free choice trials for five successive days, beginning on the day following the last day of the training series. At least 15 min. intervened between successive trials during the test series.

The motivation of the animals during the test series was hunger (22½ hours of food deprivation). They were satiated for water.

Just prior to the test series the *S*s of the various groups were divided into four sub-groups; the constant and the switched group. For the constant group all the containers on the side which had the four glasses with food during the terminal part of the training series contained Purina dog checkers during the test series. For the switched *S*s, food was placed on the side opposite to the one containing food during the latter part of the training series.

The *S*'s choice was final, once it passed either of the doors on both sides of the choice point. If it went to the side which contained food it was permitted to eat for 20 sec. If it made an incorrect response, it was kept in the goal box with the empty glasses for an equivalent period of time.

The daily experimental session occurred between 8:00 A.M. and 1:00 P.M. during the summer of 1949. The temperature during the experimental session was usually about 85° F.

Results and Discussion

Training series

The *S*s' behavior during the training series was similar to that reported by Kendler and Mencher. In the goal box the typical *S* would look into one of the glasses. If the glass contained water, the *S* would drink. If not, the *S* would proceed to the next glass, and so on, until it came upon the water-filled glass. During the latter trials of the training series the rapidity of the rats' discriminatory behavior increased. There appeared to be no observable change in the *S*s' behavior following their first 'perception' of food during the sixth or seventh day of the training series.

The effects of the massed training appeared to be fairly satisfactory in the sense that approximately half of the responses of groups 1M and 2M was to the food side. (It will be recalled that the *S*s had four trials

TABLE I

NUMBER AND PERCENT OF RESPONSES OF VARIOUS GROUPS ON FIRST TEST TRIAL TO SIDE WHICH LED TO FOOD DURING LATTER PART OF TRAINING SERIES

Group	*N*	Number of *Ss* Responding To Food Side	% of *Ss* Responding To Food Side
1F	8	5	63%
2F	8	4	50%
1F + 2F	16	9	56%
1M	7	2	29%
2M	8	3	38%
1M + 2M	15	5	33%
Total	31	14	45%

daily.) On the seventh training day, Group 1M had a mean of 2.43 responses to the food side. Group 2M had a mean of 4.25 responses during the sixth and seventh training days to the side possessing the four glasses filled with food. The range of responses for Group 1M was 1 to 3, while the range for Group 2M was 1 to 8.

Test series

Table I presents the number of responses on the first test trial to the side which led to food during the latter part of the training series. All the percentages fall within the area of chance variation. Since the *Ss* chose the 'food side' 45 percent on the first trial of the last training day, the shift from the thirst to the hunger drive did not produce any modification in their choice behavior. The results, therefore, fail to provide any evidence of the animals' reacting appropriately to their new hunger drive by choosing the side which contained food during the training trials. These findings are in essential agreement with those of Kendler and Mencher (2) and Grice (1).

Table II indicates the number of correct responses made by the constant and switched groups during the series of 20 test trials. One would expect, if any perception of the food had been formed, that the constant group would have performed at a superior level during the test series. This expectation was not fulfilled. Again, we find that the results do not support the assumption that any 'cognitions' of the location of food were formed during the training trials. Only the difference between the constant and switched animals of Group 2M was significant. That difference was significant at the five percent level. In view of the absence of any significant findings on the first test trial and for four of the five comparisons during the test series, it seems reasonable to assume that this difference at the five percent level was due to chance variation.[2]

TABLE II

THE MEAN NUMBER OF CORRECT RESPONSES DURING THE TEST SERIES FOR THE CONSTANT AND SWITCHED SUB-GROUPS

Group	Constant		Switched	
	M	σ	*M*	σ
1F	16.00	2.00	17.00	1.87
2F	18.25	.83	16.75	3.96
1M	14.33	2.62	15.75	2.77
2M	18.50	1.12	16.25	.83
Total	16.93	2.40	16.44	2.67

[2] The performance of those 'constant' animals having two days of experience with food (Groups 2F and 2M) was superior (at the one percent level of significance) during the test series to the 'constant' animals of Groups 1F and 1M which experienced food for only one day. This would suggest that *two days* of 'seeing' food was more conducive to the formation of a perception than was one day. One would expect, therefore, that the 'two day animals' in the switched sub-groups would not perform as well as the 'one day animals.' This was not found to be true. There was also no essential difference between the per-

In the present investigation, the experimental situation was arranged to preclude the possibility of the perception of the location of food from being 'experimentally extinguished.' No evidence of the Ss having learned the location of the food was obtained. The findings, therefore, do not lend any support to Professor Woodworth's specific hypothesis as to why both Kendler and Mencher (2) and Grice (1) failed to find any evidence of their animals learning the location of food under a '22 hour' thirst drive. The results also suggest that the system of forced trials used by both Kendler and Mencher (2) and Grice (1) did not influence their results.

Although the results are not consistent with Professor Woodworth's *specific* hypothesis, care should be taken in their more generalized interpretation. They do *not* imply that perceptions may not be experimentally extinguished, nor do they preclude an adequate perceptual-reinforcement formulation.

The present authors feel that there is (or should we say, need be?) no basic qualitative difference between a perceptual and S-R 'reinforcement' theory. Both approaches should have as their goal the economical description of learning phenomena. It may be that as a perceptual-reinforcement theory develops, it will be advantageous to assume that perceptions of certain objects can only be formed when they are, directly or indirectly, related to some consummatory responses involving that object.

Summary

The present investigation was designed to examine Professor Woodworth's hypothesis as to why animals of both Kendler and Mencher (2) and Grice's (1) studies failed to form perceptions of the location of food when motivated by a 22-hour thirst drive. Woodworth's hypothesis was that in these studies the perception of food was formed, but had become extinguished through repeated occurrences of seeing food without any food reward.

This study was a modification of the one reported by Kendler and Mencher. The major change was the introduction of food into one of the end-boxes of a single-unit T-maze during the latter part of the training series. Another modification was that a massed training technique was substituted for a 'forced trial' technique for half of the animals. The results of the test series, during which the animals were hungry, provided no evidence of the animals having formed a perception of the location of food when motivated by a 22-hour thirst drive.

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formance of the two day and one day animals on the first test trial.

A RE-EXAMINATION OF THE EFFECT OF MONETARY REWARD AND PUNISHMENT ON FIGURE-GROUND PERCEPTION

BY IRVIN ROCK AND FREDERICK S. FLECK *

The Graduate Faculty of Political and Social Science, New School for Social Research

Introduction

In view of the increasing interest in the relationship between cognitive and conative processes (5), it is important at this time to make sure that the results of experiments reported are reliable in the sense of being duplicable. Recently, Carter and Schooler (2) repeated the now well-known experiment by Bruner and Goodman (1) in which rich and poor children judged the size of coins. Their results did not confirm the findings of Bruner and Goodman as regards *perceptual* differences between the two groups, so that it is now far from certain that the need for valued objects operates to increase their phenomenal size in direct perception, since otherwise the minor changes in procedure reported would not abolish the effect.

Another well-known experiment, by Schafer and Murphy (4), utilized an ambiguous (reversible) figure-ground drawing as a test stimulus. Subjects were shown single faces tachistoscopically many times, some of which were consistently rewarded and some consistently punished by giving or taking away money after every presentation. The *S*s were simultaneously told the name of each face. In the test situation, rewarded and punished faces were combined so as to form an ambiguous situation in which either face could be seen as figure, while the other presumably would go unnoticed as ground in the brief presentation. The hypothesis of autistic perception advanced by the authors held that the need to receive money (reward) and conversely the need not to lose money (punishment) would operate as determinants in selecting the particular figure perceived so that the previously rewarded one should be recognized more frequently than the previously punished one in the ambiguous situation. The results indicated a significantly greater identification of rewarded faces than punished faces in the first 16 test trials. Although 32 test trials were given to each *S*, the authors did not include the latter 16 trials in their statistical computation because they believed that a set began to operate at this point in the test series, thereby introducing a factor alien to the hypothesis being investigated. It has been pointed out, however (3), that such a procedure is statistically unjustifiable. Nevertheless the results obtained for the first 16 trials cannot be ignored. In order to determine if the latter results were reliable, a repetition of the experiment with a larger number of *S*s was considered necessary.

Subjects

The *S*s for the experiment were four males and nine females, all volunteers, ranging in age from 15:8 to 20:6, with an average age of 18:00. The youngest *S* was in the fifth term of high school and the oldest in the second year of college. Nine of the 13 *S*s served as experimental and four as control *S*s.

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Procedure

The procedure of Schafer and Murphy (4) was followed as closely as possible, so that only a summary will be presented here. Certain minor changes will also be noted.[1]

Training.—The training consists of 100 tachistoscopic presentations, 25 of each of four different semi-circular profile faces (4, Fig. 1, p. 336), in random order. Just before each presentation, *S* is told the name of the face, and instructed to repeat the name aloud when the face is shown. He is then told to take or return the appropriate amount of money (either two or four cents) that he wins or loses. Each experimental *S* is told in advance that when either one of two of the four faces is shown, he will receive money and that when either one of the remaining two faces is shown he will lose money. In order to create the impression that *S* in some way determines what face will next be shown—and, therefore, his reward or punishment—he is further instructed to guess a number from one to four before each face is shown. During the first eight trials only, the further precaution is introduced of pointing out to *S* the appropriate one of four drawings of the faces placed before him which looks like the face he will next be shown by tachistoscope. The drawings were made to look like the training faces, but were 'improved' in the sense that they were 'better' faces and, therefore, absolutely non-reversible.

To summarize, the procedure included the following steps: 1. *S* guesses a number from one to four. 2. *E* points to the appropriate 'help' face. 3. *E* announces the name of the face to be shown by saying, e.g., "B." 4. The figure is shown tachistoscopically. 5. *S* repeats the name of the face—e.g., "B." 6. *E* indicates the amount of money won or lost—e.g., "You win two cents," and remits. Step 2 is eliminated after the eighth trial of the training series.

After the 50th trial, a five-min. rest period is introduced. After the 80th trial, the *S* is told that during the remaining presentations new and unfamiliar faces might be shown in order to see if he has learned the faces and their names and that he will be penalized 10 cents if he does not say "wrong" when the *E* announces such a face with the name of one of the four training faces. Actually, only one such face is introduced after the 93rd trial and was sufficiently different from the training faces to be unmistakable if the *S* really knew the faces by that time. Whereas Schafer and Murphy report that all *S*s immediately recognized this figure as strange (and presumably did not respond with "strange" to any of the other faces after the 80th trial), one of our *S*s incorrectly repeated "B" when shown the new face. In addition, most of our *S*s said "strange" to several of the training faces after the 80th trial. The latter *S*s were not penalized 10 cents and no reward or punishment was announced in such instances. The *S* who called the *new* face "B," was, however, penalized 10 cents.

The distribution of the rewarded and punished faces for the different *S*s was as follows: of the nine experimental *S*s, five were rewarded when shown Face A and Face D, and punished when shown Face B and C. The remaining four *S*s were rewarded when shown Face B and Face C and punished when shown Face A and Face D.[2] (Face A—right-pointing—and B—left-pointing—comprised the A–B ambiguous situation; Face C—right-pointing—and D—left-pointing comprised the C–D ambiguous situation). This distribution was intended to control the possible influence of structural and positional differences.

The procedure is the same in all respects for the control group except that all reference to reward and punishment is eliminated.

Post-training.—The post-training series follows after a five-min. rest period. It consists of 32 tachistoscopic presentations, 16 of each of two ambiguous (reversible) figure-ground drawings (4, Fig. 2, p. 337) in random order. Each ambiguous situation consists of one previously rewarded and one previously punished training face merged within a full circle so as to share the same contour. The *S* is instructed to respond with the correct name of the face he sees or, since he was told that some of the faces to be presented would be strange, to say "X" if he does not recognize a face as one of the four shown previously. After every presentation of an ambiguous test figure, either one or the other of two new figures is shown, depending upon *S*'s previous response. Each of these figures (4, Fig. 3, p. 338, here called E and F) resembles the ambiguous test figures in that it contains a profile line within a circle, but differs from them in that it contains only one 'good' face. If the *S*'s previous response is a left-pointing training face, then it is followed by the new face which is necessarily right-pointing (E), and vice versa. This precaution was introduced in order to prevent the development of any kind of direction set in the post-training series. Since one or the other of the new figures followed every presentation of an ambiguous test figure

[1] A copy of the full procedure, including instructions to *S*s, used in the present experiment is available on request from the authors.

[2] Actually only six of the nine experimental-*S*s learned the names of the faces, so that the results of the other three *S*s were not included in the statistical comparison. Of these six *S*s, an equal number were rewarded to Faces A and D and to B and C.

except the last, there was a total of 63 post-training trials consisting of 16 of each of the two ambiguous figures and 31 new or 'set-breaking' figures. The procedure for the post-training series is the same for experimental and control groups.

In the present experiment, certain minor changes or additions in procedure were introduced as follows:[3]

1. In order to alleviate feelings of nervousness, the *S*s were put at ease at the outset by explaining that this was not to be a test of any kind.

2. Instructions to *S*s were always read twice in order to insure full understanding of their task.

3. The experimental *S*s were given their 'winnings' directly after the end of the training series.

4. In order to facilitate learning and to eliminate possible affective connotations of familiar names, the letter names A, B, C, and D were used as the names of the four training faces.

5. In the post-training series, *S* was given the additional alternatives of responding with a question-mark if he felt he had no idea what face he had been shown, or of responding with a face name or "X" plus a question-mark if he thought he knew but wasn't really certain. As it turned out, most *S*s either forgot the instructions regarding the use of the question-mark or preferred to guess without indicating they were guessing. That this was the case is clear since very few question-mark responses were given despite the fact that the *S*s' behavior and subsequent admissions indicated that very frequently they were quite uncertain of what they had seen.

6. Certain difficulties developed with the procedure described above of following each ambiguous situation with a 'set-breaking' figure. Many *S*s called A–B or C–D figures "X." Consequently, since it was not clear whether the *S* saw the left or right profile as a strange face, is was not certain whether then to show the E or F 'set-breaking' figure. After such cases, an attempt was made to equalize the number of E and F figures shown. Occasionally, an *S* called an A–B figure C or D or a C–D figure A or B. In such cases it was assumed that the face seen was correctly localized so that, e.g., a "C" response to an A–B figure was followed by the F figure since C faced to the right and F to the left. It also frequently happened that an *S* called the E or F figure A, B, C, or D.

7. In order to determine whether or not the *S*s had actually learned the names of the four training faces, a learning test was introduced after the experiment proper was over. It consisted of showing the single faces again—with appropriate instructions—until *S* achieved 10 consecutive errorless trials or until it was clear that *S* did not know the correct name of each of the four faces. Schafer and Murphy did not employ any such independent test of learning beyond the single catch test trial described above, presumably assuming that if an *S* had not learned the names of the faces during training it would be revealed by his responses during the post-training series.

8. A post-experimental interview was included in the present experiment consisting of a series of questions concerning *S*'s experiences during the training and post-training series, particularly as regards his reaction to the monetary reward and punishment, the operation of preferences if any, and his awareness or lack of awareness of more than one face in the reversible test figures.

9. In the present experiment the tachistoscope consisted of a shutter set for a ½-sec. exposure mounted in front of an opaque projector behind and slightly to one side of *S*. (Schafer and Murphy used a Whipple tachistoscope also set for a ½-sec. exposure. Their figures were two to three in. in diameter at a distance of about 18 in. from the *S*.[4]) The projector cast an image 12 in. in diameter. *S* was seated at a distance of approximately 54 in. from a white paper screen mounted on the wall. Half of the *S*s sat slightly to the left and half slightly to the right of the screen.

Results

Experimental group.—Only six of the nine experimental *S*s demonstrated by 10 consecutive errorless trials in the final test of learning that they could correctly identify each of the four training faces by name. Since the only criterion of what an *S* perceives in the test series with ambiguous figures is the name he announces, one cannot tell what he is perceiving in that situation if he does not know the correct name for each face. Consequently, only the results for the six *S*s who did learn the names of the faces will be considered. The fact, however, that three of the nine

[3] In some instances it is not certain whether these were actually modifications, since the description of the procedure employed by Schafer and Murphy is not explicit on all points.

[4] These data were supplied in a personal communication by Dr. Schafer.

TABLE I

NUMBER OF RESPONSES TO POST-TRAINING TEST FIGURES: ENTIRE SERIES
EXPERIMENTAL GROUP
(See RESULTS for meaning of symbols.)

Condition	Subject	Responses to A–B and C–D Figures						Responses to E and F Figures				Total Number of:		
		R	P	X	?	M	N	X	M	?	N	Correct Responses	Incorrect Plus Question-Mark Responses	N
A and D +, B and C–	Josephine	14	2	5	0	11	32	14	17	0	31	30	33	63
	Joel	10	1	14	0	7	32	0	31	0	31	11	52	63
	Lois	7	16	6	2	1	32	22	8	1	31	45	18	63
B and C +, A and D–	Lenore	10	18	2	2	0	32	27	3	1	31	55	8	63
	Michael	21	9	0	2	0	32	29	0	2	31	59	4	63
	Ernest	8	18	5	0	1	32	31	0	0	31	57	6	63
	Sum	70	64	32	6	20	192	123	59	4	186	257	121	378

experimental *S*s and one of the four control *S*s failed to learn the names of the faces is, in itself, of interest, since Schafer and Murphy do not report any such cases.

The results for the six *S*s are given in Table I. The responses to the ambiguous A–B or C–D figures are indicated in terms of the number of times each *S* responded with the name of a previously rewarded face (R, column 3) and with the name of a previously punished face (P, column 4). Column 5 indicates the number of times each *S* called the A–B or C–D figure strange (X). Such responses are, of course, wrong. Column 6 shows the number of times each *S* responded to the A–B or C–D figures with a question mark.[5] Column 7 gives the total number of incorrect responses (M = mistakes) to the A–B and C–D figures excluding X responses. Thus, responses of A or B to the C–D figure and C or D to the A–B figure are included. In column 8 the total number of responses is presented. The next three columns indicate the distribution of X, M, and ? responses, respectively, for the E and F figures. This information is included here in order to give the full picture of how each *S* responded to the entire 63 test trials. An X response here is, of course, correct. An M response implies that the *S* called the E or F figure A, B, C, or D. The next to the last two columns summarize the foregoing data in terms of total number of correct responses as against the total number of incorrect plus question-mark responses for all 63 trials. The total number of correct responses is derived by adding the number of R and P responses to the ambiguous figures to the number of X responses to the E and F figures. All other responses are not correct. These figures, although not immediately relevant to the main result of R vs. P responses, are included because they point up the fact that although all six *S*s had learned the names of the faces, there nevertheless remained a great deal of uncertainty,

[5] In those few cases where the *S* gave an A, B, C, D, or X response plus a question mark it was tallied as A, B, C, D, or X, respectively. Thus "C?" was counted as a C response for the trial in question.

confusion and guesswork in the post-training test series. By consulting the last row of sums it is seen that of a total of 378 trials, 121 or roughly one third of all the responses were incorrect. This result in itself is in striking contrast to the results of Schafer and Murphy where very few incorrect responses were given, although these authors do not indicate what the responses were to the 'set-breaking' figures.

As is indicated in Table I, for the two amibguous figures there was a total of 70 responses of names of faces previously rewarded as against a total of 64 responses of names of faces previously punished. The χ^2 for this difference is .268 and $P < .7$. Hence, unlike the findings of Schafer and Murphy, our data indicate no reliable difference between the number of rewarded and punished faces perceived in the ambiguous figures. Analysis of the results for the different *Ss* reveals that three of the six *Ss* perceived more punished than rewarded faces and three perceived more rewarded than punished faces. Thus, there is not any consistent trend in our data to support the conclusions of the previous authors.

A comparison of the two ambiguous situations, considered separately, is presented in Table II. As is indicated in the bottom row of sums, in the A–B situation, a total of 23 rewarded faces and 40 punished faces was perceived. The χ^2 for this difference is 4.6 and $P < .05$. Thus, there is a significantly greater tendency to see punished as against rewarded faces in the A–B ambiguous situation. On the other hand, a total of 47 rewarded faces and 24 punished faces was perceived in the C–D situation. The χ^2 for this difference is 7.4 and $P < .01$. Hence, there is a significantly greater tendency to see rewarded as against punished faces in the C–D ambiguous situation. Since the results for the two situations, considered separately, point in opposite directions, such fractionation certainly does not support the over-all thesis of autistic perception of previously rewarded figures. This result is again quite different from that reported by Schafer and Murphy where both the A–B and C–D situations showed a superiority of rewarded over punished faces perceived. On the other hand, the fact that in our data both sets of differences were *significant*, requires some

TABLE II

COMPARISON OF RESPONSES TO THE A–B AND C–D SITUATIONS: ENTIRE SERIES EXPERIMENTAL GROUP

(See RESULTS for meaning of symbols.)

Condition	Subject	Responses to A–B Figure						Responses to C–D Figure					
		R	P	X	?	M	*N*	R	P	X	?	M	*N*
A and D +, B and C–	Josephine	3	0	3	0	10	16	11	2	2	0	1	16
	Joel	1	0	14	0	1	16	9	1	0	0	6	16
	Lois	1	15	0	0	0	16	6	1	6	2	1	16
B and C +, A and D–	Lenore	4	10	1	2	0	17	6	8	1	0	0	15
	Michael	11	4	0	1	0	16	10	5	0	1	0	16
	Ernest	3	11	2	0	0	16	5	7	3	0	1	16
	Sum	23	40	20	3	11	97	47	24	12	3	9	95

TABLE III

NUMBER OF RESPONSES TO POST-TRAINING TEST FIGURES: FIRST 16 TRIALS
EXPERIMENTAL GROUP
(See RESULTS for meaning of symbols.)

Condition	Subject	Responses to A–B and C–D Figures						Responses to E and F Figures			
		R	P	X	?	M	N	X	M	?	N
A and D +, B and C–	Josephine	5	2	4	0	5	16	8	8	0	16
	Joel	5	1	7	0	3	16	0	16	0	16
	Lois	3	8	3	2	0	16	13	3	0	16
B and C +, A and D–	Lenore	4	9	1	2	0	16	14	2	0	16
	Michael	12	4	0	0	0	16	14	0	2	16
	Ernest	5	8	3	0	0	16	16	0	0	16
	Sum	34	32	18	4	8	96	65	29	2	96

explanation. In examining the individual results for both situations combined (Table I), it appears that, although the total number of rewarded and punished faces perceived is about equal, this total derives from highly *unequal* distributions for each *S*. Thus, e.g., Josephine perceived 14 rewarded and 2 punished faces, Lois 16 punished and 7 rewarded faces, etc. Pursuing this analysis further, one finds that such disparities for each *S* derive from the particular dominance of response of either *one* rewarded face or *one* punished face. Thus, of Josephine's 14 responses to rewarded faces, 11 were to Face D and only 3 to Face A; of Lois' 16 responses to punished faces, 15 were to Face B and only 1 to Face C (see Table II). Hence, it is suggested that the significant differences found by comparing rewarded to punished responses in the A–B and C–D situations separately are, in a way, artifacts, resulting from the greater dominance of the punished face for several *S*s in the A–B situation and of the rewarded face for several *S*s in the C–D situation. The fact of such response-dominance is discussed further in the following section.

The results for the first 16 trials considered alone are presented in Table III. As is indicated in the bottom row of sums, 34 responses of rewarded faces and 32 of punished faces were given to the A–B and C–D ambiguous situations during the first 16 presentations of these figures. The χ^2 for the difference is .06 and $P < .8$. Hence, according to the present data, regardless of whether some kind of set or perseverating tendency developed after the 16th trial, the hypothesis of autistic perception of previously rewarded faces is not confirmed by the results of the first 16 trials. Furthermore, of the six *S*s, the same three who gave more 'punished' than 'rewarded' responses for the entire series gave more 'punished' than 'rewarded' responses for the first 16 ambiguous presentations.

Control group.—In view of the possibility of competition between structural and conative influences, the use of control *S*s who are neither rewarded nor punished in the training period becomes important. Schafer and Murphy included one control *S* but did not report the results obtained. Of the four control *S*s included in the present study, only three demonstrated that

TABLE IV

NUMBER OF RESPONSES TO POST-TRAINING TEST FIGURES: ENTIRE SERIES CONTROL GROUP

(See RESULTS for meaning of symbols.)

Subject	Responses to: A–B Fig. and C–D Fig.								Responses to E and F Figs.				Total Number of:		
	A	B	C	D	X	?	M	N	X	M	?	N	Correct Responses	Incorrect Plus Question-Mark Responses	N
Helen	0	0	14	2	10	0	6	32	12	16	3	31	28	35	63
Edith	6	0	3	0	17	0	6	32	14	17	0	31	23	40	63
Marie	12	3	0	4	13	0	0	32	17	14	0	31	36	27	63
Sum	18	3	17	6	40	0	12	96	43	47	3	93	87	102	189

they had learned the names of the faces in the final test. Table IV gives the results for these three *S*s in terms of the number of correct A and B responses to the A–B figure and the number of correct C and D responses to the C–D figure. Also given are the total number of strange (X), question-mark (?), and incorrect (M) responses to the A–B and C–D situations, as well as the breakdown of responses to the E and F figures, as in Table I. The total number of correct as against incorrect plus question-mark responses to all 63 trials is given in the next to the last two columns. As is indicated in the bottom row of sums, there was a total of 18 'A' and 3 'B' responses to the A–B situation, 17 'C' and 6 'D' responses to the C–D situation. These figures are, of course, too small to draw any reliable inferences concerning structural differences. It may be noted, however, that two of these three *S*s gave a preponderance of responses to one of the two faces in *one* ambiguous situation, namely Helen with 14 C to 2 D responses and Marie with 12 A to 3 B responses.

Of the total of 189 responses given by the three control *S*s to the 63 trials, 87 were correct and 102 were either incorrect or question-mark responses. Thus, for the control *S*s more than half of all the responses in the test situation were not correct despite the fact that these *S*s shortly thereafter demonstrated by 10 consecutive errorless trials that they did know the correct name for each face.

Interview.—By including an interview in preliminary experiments, certain facts emerged which otherwise would not have been ascertained. For example, several *S*s reported having immediately seen *both* faces—or at least *two* faces—in many of the ambiguous presentations, while nevertheless responding with the name of only one face.[9] Such a fact is important because it indicates that the verbal response need not always accurately describe the percept.

The data obtained from the post-experimental interviews can be summarized as follows: Only one of the six experimental *S*s (Michael) actually experienced 'winning' and 'losing' money with any degree of involvement. All other *S*s claimed that the money meant nothing to them but instead that they were anxious to "do well" and to learn what they were supposed to learn. Some *S*s

[9] This only occurred once for one *S* and several times for another *S* in the experiment proper. The reason for this difference, it is believed, is that a more accurate timing device was introduced.

were actually insulted by the use of money as a reward and punishment and refused to take their 'winnings' afterward until the *E*s insisted. Thus, e.g., one *S* in answer to the question "How did you feel about the money?" said, "It didn't make any difference, I just kept pushing it back and forth." Another *S* said, "I was indifferent to it." When pressed further, he added: "Naturally everyone prefers to win, but in this case it didn't mean anything." Another *S* said: "Yes, I wanted to figure out why I won it, not for the money itself but to get the formula of the numbers. The money didn't mean anything. I was more interested in the facts behind it than the money itself."

The one *S* who apparently was involved with the money gave 21 'rewarded' and 9 'punished' responses to the A–B and C–D ambiguous situations in the post-training series. Thus, it is possible that for this *S* the greater number of 'rewarded' as against 'punished' responses given to the ambiguous situations can be attributed to the selective influence of prior reward and punishment. On the other hand, during the interview he reported that during the post-training series he saw two faces on several occasions. He then went on to say:

> I think I decided on one of the two at the beginning. Then when the same picture was flashed again, I gave that same answer, whether initially I was wrong or right. I had no better way of identifying them. I found it difficult to see them when in a complete circle.

In the case of all other experimental *S*s, however, there is reason to doubt that the intended situation of reward and punishment existed. Not only did these *S*s deny having experienced any reward or punishment but four *S*s did not know which faces had been rewarded and which punished. One *S* knew which was which for one ambiguous situation but not for the other. Furthermore, the four *S*s who did not know which faces were rewarded and which punished did not even believe that certain faces *were always* rewarded and certain faces *always* punished. This is understandable in view of the fact that they were preoccupied during the learning series with learning the names of the faces and with trying to figure out the 'system' behind the number-guessing procedure. They did not attend to the relationship between a particular face and the fact that it was rewarded or punished. In view of these facts, if the reward and punishment were at all factors in creating differential attitudes towards the faces, they must have operated on a mechanical and nonconscious level.

Each experimental *S* voiced a particular preference or dislike for one or more of the faces. With the exception of Michael and Josephine, however, there is no clear relationship present between indicated preference and number of corresponding responses to the ambiguous situation. There is, however, for some *S*s, a relationship between such stated preferences and the total number of times that particular face was given as a response—correctly or incorrectly—during all 63 trials. Joel, e.g., who said he preferred B, called every F Figure "B" throughout the series. In fact, an *S*'s indication of a preference may very well be the result of recalling his own responses during the post-training series. If this is true then such stated preferences are of little interpretive value since they would be more the result of, rather than a cause of, *S*'s own responses.

There was only one case where anything like a volitional effort or expectation operated to influence the perceptual 'selection,' according to the interview-protocols. This was where the *S* (Lois) maintained that she always looked for faces looking towards the left. This contention is borne out by her results, since she saw B 15 times and D six times whereas A and C were seen only once each.

Discussion

The results of this experiment do not substantiate the previous findings of Schafer and Murphy (4) that differential monetary reward and punish-

ment influence the perception of faces in a reversible figure-ground situation. An analysis of the quantitative and qualitative data obtained suggests several reasons for the present findings, involving certain weaknesses and previously undetected aspects of the procedure followed.

1. There is reason to doubt that the monetary reward and punishment succeeded in establishing the intended attitudes towards the different faces. It is possible, of course, that such attitudes were created without *S* being aware of it, so that it is difficult to demonstrate conclusively whether or not this prerequisite condition actually existed in both the original experiment and the present repetition of it.

2. Even if such attitudes towards rewarded and punished faces were successfully induced in training, however, or if it would be possible so to change the procedure that they would be induced, a purely statistical consideration of results is apt to be misleading. It was pointed out, e.g., that there was a considerable degree of uncertainty and confusion in the post-experimental test series, as is indicated by the large number of incorrect responses. Why should this be the case for *S*s who subsequently demonstrate that they can correctly identify the four faces? The answer is, the authors believe, that the ambiguous figure containing the two faces is, as a whole, phenomenally something quite different from either one of these faces presented alone. Although most *S*s did not perceive the two faces in the short exposure, still, the full circle with its dividing profile line is qualitatively different from the single closed figure of a face. The protocols of several *S*s support this contention. Thus, e.g., Michael, as noted above, claimed he found it difficult to see the face when it was in a complete circle. Joel, in describing his preference for Face A said: "I could see it more easily, but in the full circle, I didn't recognize it frequently." Another *S* referred to the "jumbled up" appearance of the ambiguous figures. The most conclusive evidence on this point, however, came from the reaction of several *S*s when *E* was explaining the purpose of the experiment after it was over. At that time, when *S* was shown that two training faces were both present in the ambiguous figure, many *S*s denied that they looked the same as the single faces even though they were both present for direct comparison. The reader can get some idea of such differences by comparing the single faces used in the training series with the faces in the ambiguous situations (4, Figs. 1 and 2, pp. 336, 337). Tachistoscopic presentation probably increases such phenomenal differences.

Another source of the difficulty in correctly identifying faces in the post-training series is the introduction of the two new faces, E and F. It will be recalled that in the training series, the mere instructions that new faces might be shown (after the 80th trial) resulted in quite a few incorrect responses of "strange" to the A, B, C, and D faces.

3. These facts, in turn, are related to another aspect of the results, namely the effect of the *sequence* of trials in the post-training test series. If the interpretation given above is correct, then the post-training series represents a situation where the previous learning is not necessarily immediately helpful. The relatively new appearance of all faces in the ambiguous setting implies that *S* is not at all certain of what is being presented. A certain amount of guesswork is therefore involved at the outset for many *S*s.

After a while, however, all of the ambiguous and 'set-breaking' figures have been presented, so that *S* can compare one with another from memory and arrive at a decision. The new global impression of the A–B Figure is certainly different from that of the C–D, E, or F Figures, so that, in a sense, a new process of discriminative learning is going on in this series. Once decisions are reached as to how each figure is to be identified, the *S* usually sticks to them for quite a number of trials, or, in some cases, for the remainder of the series even though he realizes he may be wrong throughout. There is, in other words, a certain consistency which develops in responding to a situation where there is no check on correctness or incorrectness after each trial. The writers suggest that this explanation accounts for the development of what Schafer and Murphy called a 'set.' If this explanation is correct, then one must be cautious about statistical interpretations of the quantitative results of the post-training series. Differential total scores would then not necessarily imply so and so many recognitions of one face vs. another, but rather consistent application of decisions as to which previously learned face most resembles which new global presentation. For this reason, a better, though practically infeasible, procedure would be to give, say, 100 *S*s each one trial with each ambiguous situation in order to avoid the contaminating effect of early presentations of ambiguous figures upon subsequent ones. Of course such a procedure would not eliminate the other difficulties with this experiment.

The evidence supporting the above interpretation derives from: (1) the great number of errors—including consistently made errors—in the post-training series in the light of the subsequently demonstrated ability to distinguish the single faces when presented alone; (2) introspective reports of *S*s and like observations by the two *E*s indicating the phenomenal difference between faces seen in the two situations; (3) initial uncertainty in the post-training series indicated by delayed response, variations in responses, etc., followed by seemingly increased certainty and increased consistency of responses to each figure; (4) introspective reports by some *S*s confirming the interpretation of an attempt at consistency after an initial period of uncertainty. The process, in many cases, may have been somewhat as follows: At first, when, e.g., the A–B Figure is presented, *S* may, within the new global impression, perceive Face A looking towards the right. If he calls this figure "A," he may nevertheless have doubts as to its identity with the A figure he had learned previously. The next time the A–B figure is shown, the global impression is now somewhat familiar, and is associated with the idea of "A, facing right," derived from the previous trial with A–B, and so on. Furthermore, in the interim, he has seen other figures which he tentatively identifies as, e.g., "C" or "X," so that the comparison supports his initial assumption about "A." Although the initial 'selection' of one or the other of the two faces in an ambiguous figure may be determined by prior reward or punishment, there is no evidence of this in the results of this experiment. Certainly other factors also play a role in such determination. Once such a 'selection' is made, however, it tends to remain dominant for the reasons suggested above.

There were exceptions to this trend towards consistency of response to a given figure. Often an *S* would change his response to a figure after having responded several times in succession previously with one face-name. If this happened, it was apt to affect his responses to the other figures in order to avoid obvious self-contradiction. This is actually another kind of

consistency and again points up the fact that the response on one trial very much influenced responses on subsequent trials. There were, however, a great many other responses and sequences of responses which cannot be explained as instances of an attempt to be consistent following initial or subsequent decisions regarding the identification of the different faces. Many of these responses were no doubt instances of spontaneous figure-ground reversal.

It is regrettable that only the results for six experimental and three control *Ss* of the total of 13 *Ss* initially participating, could be included in the data examined. It would be particularly valuable in any further experimentation of this type to use a larger number of control *Ss* in order to determine the relative influence of structural differences between faces.

Summary

An experiment devised to test the hypothesis of autistic influence on figure-ground perception was repeated with nine experimental and four control *Ss*. The results can be summarized as follows:

1. Only six of the nine experimental *Ss* and three of the four control *Ss* actually learned the names of the training faces.

2. There was no significant difference between the total number of previously rewarded and of previously punished faces correctly identified in the ambiguous situations for the six experimental *Ss*. There were significant differences between the number of previously rewarded and previously punished faces correctly identified in each of the two ambiguous situations considered separately, but the differences were in opposite directions for the two situations. The difference between rewarded and punished faces identified was not significant for the results of the first half of the series of ambiguous figures considered alone.

3. A considerable number of mistakes was made in identifying the ambiguous figures.

4. Most *Ss* indicated that they did not experience the monetary reward and punishment as rewarding or punishing. Several *Ss* did not know which faces had been rewarded and which punished.

5. Evidence was cited to the effect that the faces composing the ambiguous figures appeared phenomenally different from the single faces when seen separately. It was suggested that this fact creates a condition of uncertainty during the series of ambiguous figures so that new learning processes are involved. The latter disturb the purity of this series as a test of differential figure-ground perception.

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ACQUISITION OF MOTOR SKILL: III. EFFECTS OF INITIALLY DISTRIBUTED PRACTICE ON ROTARY PURSUIT PERFORMANCE[1]

BY ROBERT B. AMMONS

University of Louisville

This paper is the third in a series of systematic studies of major variables influencing skills acquisition and performance (1, 2). In the first paper (1), a set of variables was operationally defined in terms of the skills performance curve, a tentative formulation made of a miniature system guessing at some of the relationships between these variables, and predictions made as to the outcomes of certain further experiments. In the second paper (2), a study was reported which showed the effects of the duration of an initial practice period and a single rest of variable length on subsequent rotary pursuit performance.

The key constructs used were *temporary work decrement* (D_{Wt}), *warm-up decrement* (D_{ww}), and *permanent work decrement* (D_{Wp}). They were defined roughly as follows. D_{Wt} is the amount the performance curve is depressed by factors which disappear over a rest. It is measured by introducing a rest of sufficient length to allow recovery from these temporarily depressing factors (10 or more min. in rotary pursuit), then comparing the post-rest performance corrected for D_{ww} with that of a group given no rest. The difference in favor of the rest group at the start of post-rest practice is considered to be the amount of D_{Wt}, i.e., the amount the pre-rest curve is depressed by temporary work factors. D_{ww} is the amount the performance curve is depressed by the need to warm up after a rest. It is measured as the difference between actual performance level, and the level predicted by backward extrapolation of a curve fitted to a later segment of the actual performance curve. D_{Wp} is the amount the performance curve is depressed by relatively permanent work factors. It is measured as the difference between actual performance corrected for D_{ww} and D_{Wt} and performance under optimum conditions of practice with respect to distribution. More detailed operational definitions of D_{Wt}, D_{ww}, and D_{Wp} can be found in the two previous papers.

Investigators have reported many instances of temporary or long-range superiority in performance associated with relatively distributed as compared with relatively massed skills practice (3, 5, 6, 9, 10, 11). A limited attempt was made in the first paper of the present series to predict the conditions under which and amounts by which skills performance with distributed practice would exceed that with massed. Predictions were based on the following hypothesized properties of D_{Wt}, D_{ww}, and D_{Wp}. D_{Wt} cumulates additively during practice, but dissipates in time at a rate proportional to the amount present. D_{ww} is greater the more practice, prior to the current period, the longer the interpolated rest, and the fewer the times practice has been interrupted by rests of appreciable length. D_{Wp} is greater the more D_{Wt} present and the longer it is present during practice.

In a study where durations of intertrial rest intervals were varied systematically for different practice groups during an initial practice period, the effects of different distributions could be evaluated during subsequent practice periods where all groups practiced under the same condition of distribu-

[1] This study was made during 1947 and 1948 with the assistance of the Bureau of Research in Humanities and Social Development of the University of Denver. Thanks are due Mr. Cliff Wing and Mr. Wilbur Hubbard, who aided in the collection and analysis of the data, and Dr. Dean Foster, who read the manuscript critically.

tion. The following performance outcomes can be predicted from the above variables and their relations as postulated and empirically determined (1, 2):

(a) Increase in duration of intertrial rest up to about five min. will lead to better immediate performance. Rests of greater duration give more time for dissipation of D_{Wi} between trials. Since D_{Wi} is almost completely dissipated over a five-min. rest (2), little or no D_{Wi} will carry over from practice period to practice period separated by five-min. or longer rests.

(b) Increase in duration of intertrial rest will give diminishing returns with respect to immediate performance, the longer the intertrial rest. This outcome is derived algebraically elsewhere (1).

(c) Increase in duration of intertrial rest up to about five min. will lead to an increase in subsequent performance level corrected for D_{ww} and D_{Wi}. Less D_{Wp} will develop, since less D_{Wi} will cumulate.

(d) Increase in duration of intertrial rest will give diminishing returns with respect to subsequent performance level corrected for D_{ww} and D_{Wi}, the longer the intertrial rest. Since less D_{Wi} cumulates (see Prediction b), less D_{Wp} will be built up.

(e) Increase in duration of intertrial rest up to five min. will lead to less D_{ww}. Longer rests give more experience in building up set each time practice is resumed. Since D_{ww} is maximal (set is completely lost) over a five-min. rest (2), practice in reinstating it should be just as effective over a longer rest, and more effective than over shorter rests.

(f) The greater the relative or absolute loss in performance level during a practice period, the greater the gain over the following rest. Depression of performance is postulated as due to cumulation of D_{Wi}. D_{Wi} is dissipated over a rest. Therefore amount of loss during a practice period and gain over a subsequent rest should be closely correlated.

In most respects these predictions could also be made from the theories of Hull (4) and Kimble (5) which are similar to the formulation used in the present series of papers.

Problem

This study is designed to determine empirically the effects of differences in distribution of practice during an initial practice period on (a) performance levels during this period, (b) warm-up decrements after a rest, and (c) subsequent stable performance levels when temporary work decrement and warm-up decrement are equated for all groups. The data collected should make possible the testing of the six hypotheses formulated in the introductory section of this paper on the basis of the two earlier papers in the series.

Experimental Arrangements

Apparatus

Pursuit rotor apparatus like that used in the previous study (2) in this series was not available from the Air Force. A four-rotor unit was built as similar to this apparatus as possible in materials and dimensions. The turntables were of black vinylite 10¾ in. in diameter with ¼-in. brass targets set with their centers 3½ in. from the turntable centers and flush with their surfaces. The styluses consisted of a handle and extension of ¼-in. brass rod and were hinged so that only the weight of the extension (0.6 oz.) rested on the turntable. The hinged sections were 7½ in. long, with an 80 degree bend 1¼ in. from their ends. They were tipped with silver.

When a steady physical contact was made between stylus and target, a six-volt DC circuit was completed. This closed a relay which, in turn, closed the clutch circuit of a 6V DC Standard Electric .001-min. timer. Two banks of four timers each were used alternately in the circuit to permit continuous recording of scores.

Controls for turntable rotation and clock switching were manual because of the complexity of the conditions, particularly when two or more groups were being run concomitantly.

Turntables were turned by 78 r.p.m., governor-controlled phonograph motors. These were set to revolve clockwise at 60 r.p.m. and were checked at least every 40 min. of operation. Their speed was found to vary little over this period, seldom decreasing or gaining more than one r.p.m. Stylus tips and targets were cleaned approximately every 15 min. of use with crocus cloth and a rag impregnated with carbon tetrachloride. Illumination of the rotor plates was indirect, and incident light was kept above 15 foot-candles in intensity.

Rotors were arranged so that during practice *S*s stood in a straight line about three feet apart facing *E*.

Subjects

To keep the group as homogeneous as possible, only undergraduate women college students were used. A total of 212 *S*s were run, the records of 28 of whom were discarded in matching the means and variabilities of scores for the first 20-sec. practice period for the seven conditions. In addition, four records were discarded because of apparatus change which affected performance markedly, four because *S*s did not appear on time for practice sessions, and eight because they were collected with 20-sec. instead of 22-sec. trials during Period 1. Analyses are based on matched groups, with 24 *S*s in each of the seven conditions. All *S*s were volunteers who were naive to the apparatus.

Procedure

*S*s were assigned as available to the seven practice conditions. No apparent bias resulted from this method of assignment. All practiced three 12-min. periods, referred to as Periods 1, 2, and 3. Period 1 consisted of 36 22-sec. trials, with performance measured only during the last 20 sec. of each trial. A 2-sec. preliminary period was introduced at the start of every practice period in an attempt to equate conditions with respect to preparedness to practice. 36 20-sec. trials were given in Period 2 and Period 3. Thus, all groups had 108 20-sec. periods or a total of 36 min. of timed practice. Prior to each resumption of practice, a warning was given; after about three sec. the rotors were started; then after two more sec. the timers were switched into the circuit.

The seven groups differed only with respect to the intertrial rests given during Period 1. Groups practiced with 0 sec., 20 sec., 50 sec., 2 min., 5 min., 12 min., or approximately 24 hrs. between 20-sec. trials. A given group's rests were all of the same duration during Period 1. After cessation of practice at the end of trial 36 in Period 1, a rest of approximately 20 min. was given, followed by the start of Period 2 of practice. Practice was continuous during Period 2. This practice was then followed by a rest period of approximately 24 hrs., at the end of which *S*s practiced continuously once more in Period 3. Four trials a week were given the 24-hr. rest group, with any missed session made up on the fifth or sixth day of the week.

The practice conditions can be schematized as follows:

Period 1

36 22-sec. trials. Intertrial rests of 0 sec., 20 sec., 50 sec., 2 min., 5 min., 12 min., or approximately 24 hrs.

20-min. rest

Period 2

36 20-sec. trials. No intertrial rests.

24-hr. rest

Period 3

36 20-sec. trials. No intertrial rests.

It can be seen that Period 1 provides systematically different distributions of practice, while Periods 2 and 3 equate distributions for all groups, to allow measurement of the effects of the initially different distributions.

*S*s were run four at a time except in a few instances where one did not show up in time for a session. This happened frequently only in the 24-hr. group where *S*s came on 37 different days and were allowed to make up absences. Before the start of the first trial, *S*s were instructed not to expect to do very well initially but to try to and expect to improve, to hold the stylus in a relaxed grip and keep its tip in contact with the target as much of the time as possible using an easy swinging movement of the arm, and not to press down on the stylus tip in any way. A tennis-type grip was demonstrated, and each *S*'s grip checked. Deviations from instructions during practice were corrected verbally by *E*.

Directly encouraging remarks were made about once every 60 sec. of practice. This interval was adhered to strictly with all groups, since it had been noticed in preliminary work that *E*s tended to encourage distributed practice groups more frequently. It may be that amount of encouragement was still not completely controlled, since some *S*s were apparently motivated merely by instructions to resume practice.

*S*s talked, read, or studied during the 20-sec., 50-sec., and 2-min. rest periods; played cards or read during the 5-min., 12-min., and 20-min. rest periods; and carried on their normal activities during the 24-hr. rest periods. Where rest pe-

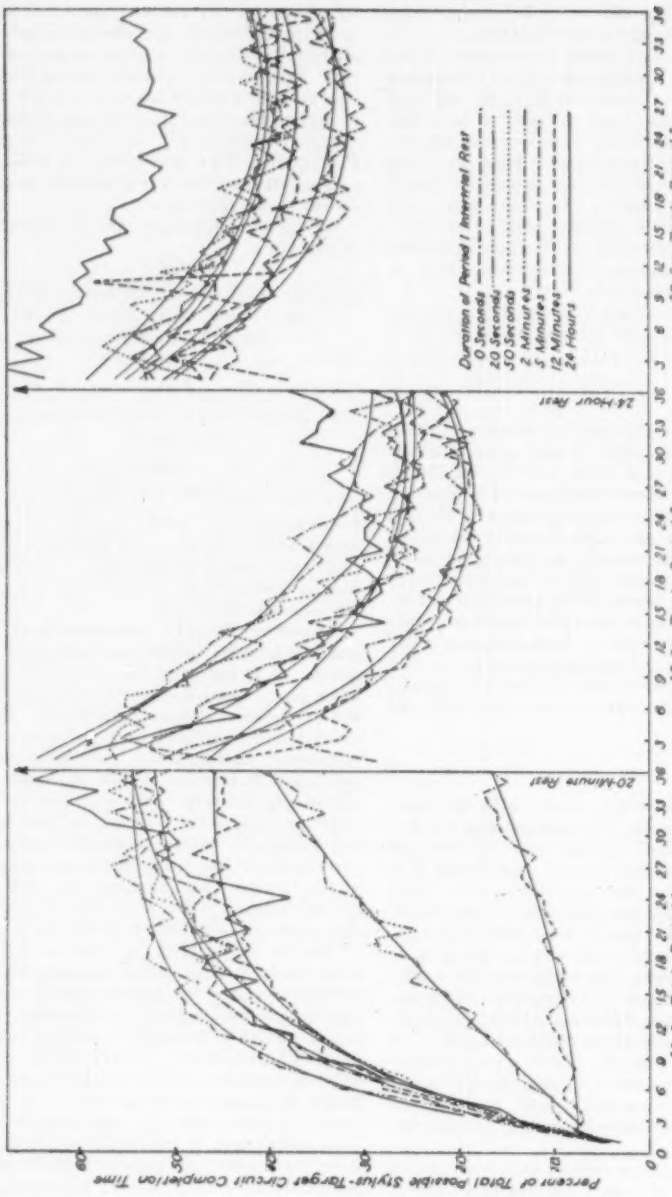

FIG. 1. Performance curves for all groups, based on mean scores by 20-sec. trials

riods were 50 sec. or longer, two or three groups were sometimes run at the same time, one group practicing during the rest periods of the other. *Ss* knew that the test had been used by the armed forces to measure coordination. They were given only very general information about their performance, usually that it was 'average' or 'somewhat above average.'

Results

Two methods of data analysis are used. The principal one is to plot group mean clock scores by 20-sec. trials, and fit these curves visually with smoothed curves corrected for warm-up decrement (D_{wu}). These smoothed curves are then used to obtain estimates of initial and final performance levels within periods, temporary work decrement (D_{Wt}), and warm-up decrement (D_{wu}). All estimates of the effects of distribution are based upon the smoothed curves which provide more stable estimates than any given points on the actually obtained curves. The second, supplementary, method involves analysis of variance of raw scores at various points in practice.

It should be noted that the scores are times of stylus-target electrical circuit completion. Wing and Ammons (12) found that these scores were not equivalent to time on target, but more nearly to *steady* time on target.

General characteristics of performance

Mean scores by 20-sec. trials for all groups for Periods 1, 2, and 3 are shown in Fig. 1. Smoothed curves disregarding D_{wu} are also given. All curves rise during Period 1, and fall during Periods 2 and 3. Performance at the start and finish of Period 3 is higher than at corresponding points in Period 2. The gain from Period 2 to Period 3 is not great. From the initial points in practice it can be seen that most of the learning took place in Period 1. There is less loss during Period 3 than Period 2, as predicted (1), presumably because less D_{Wt} is accumulated as *Ss* become more proficient. No ceiling effect is apparent, since even at their highest point, at the start of Period 3, mean scores are only 39 to 67 percent of the possible maximum. During Period 3, the groups maintain fairly stable positions relative to each other, indicating that some sort of fairly permanent effect has appeared.

Although the mean scores for the 24-hr. group are shown in Fig. 1, no further analysis is made of the performance of this group for several reasons. The performance was atypical in that initial scores were so low that they could not be matched with those of other groups, a large jog appears in the curve in the middle of Period 1, the curve wanders considerably in Period 2 but is generally not higher than the curves of other groups, and the curve in Period 3 suddenly is much higher than those for all the other groups. This group was run at a different time in the year from the others, and initial instructions were somewhat different. Practice was over approximately a 61-day period, while other groups practiced on two consecutive days only.

Final performance levels in Period 1

Fig. 2 shows the initial and final levels reached in all three periods, taken from the visually smoothed curves where D_{wu} is corrected for. Our first concern is with performance levels at the end of Period 1. Best performance appeared with 50-sec. and 2-min. rests. Shorter and longer rests than these gave poorer performance. These findings agree with those of Spence, Buxton, and Melton (10),

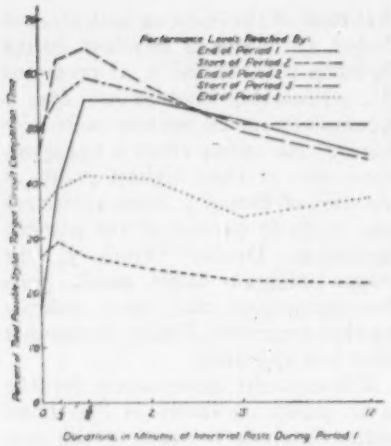
Performance Levels Reached By:
End of Period 1
Start of Period 2
End of Period 2
Start of Period 3
End of Period 3
Percent of Total Possible Stylus - Target Contact Completion Time
Durations, in Minutes, of Intertrial Rests During Period 1

FIG. 2. Initial and final estimated performance levels as related to initial distribution of practice

who report that *Ss* performed progressively better as intertrial rests were increased from 0, to 5, to 10, to 20 sec. for different groups. Their longest rest was 20 sec. That poorer performance might be observed with longer rest periods is suggested by Travis' work with the pursuit oscillator (11). He found that *Ss* performed better with a 20-min. rest between 5-min. practice periods than with 48-hr., 72-hr., or 120-hr. rests.

Differences in final performance levels in Period 1 have been attributed theoretically (1) to differences in amounts of cumulated D_{Wi}, less cumulating with longer rests allowing for dissipation. Examination of Fig. 3A shows that D_{Wi} was progressively greater for groups having 2-min., 50-sec., 20-sec. and 0-sec. intertrial rests, as compared with the groups with 5-min. and 12-min. rests. This agrees with findings in a previous study (2). The 5-min., and 12-min. intertrial rest groups apparently built up no D_{Wi} during Period 1 practice. Since the final performance levels of the 50-sec. and 2-min. groups were the same, and were higher than those of the 5- and 12-min. groups, performance level cannot be completely accounted for in terms of amounts of D_{Wi} cumulated. Prediction (a) is only partially supported, but Prediction (b) is borne out.

Although D_{Wi} and performance level are correlated up to 50 sec. of intertrial rest, some factor other than D_{Wi} seems to have depressed the final performance levels of the 2-min., 5-min., and 12-min. groups. The amount of depression is progressively greater, the longer the intertrial rest. Inspection of curves showing D_{wu} and D_{Wp} as a function of distribution (Figs. 2 and 4) indicates that changes in them are not directly related to this depression.

Initial and final performance levels in Periods 2 and 3

Fig. 2 shows the initial and final performance levels in Periods 2 and 3, as estimated from the smoothed curves and thus corrected for D_{wu}. The initial levels can be used as indicators for D_{Wp}, since D_{Wi} as well as D_{wu} is corrected for. The final levels can also be used if it is assumed that approximately the same amount of D_{Wi} is built up in all groups. That this is approximately the case is shown in Fig. 3A by the curve giving D_{Wi} at the end of the second period.

All curves show optimum performance (minimum D_{Wp}) following an intermediate level of distribution. The best performance is by the 50-sec. group at the start of Period 2, the 20-sec. group at the end of Period 2, the 50-sec. group at the start of Period 3, and the 50-sec. group at the end of Period 3. All curves show poorer performance by the 5-min. and 12-min. groups.

Predictions (c) and (d) state that D_{W_2} will be proportionate to the amount of D_{W_1} built up and how long it remains during practice. If performance levels at the start and finish of Period 3 are taken to indicate amount of D_{W_2} built up during Period 1, Prediction (c) holds for the 0-sec., 20-sec., and 50-sec. groups, but not for the 2-min., 5-min., and 12-min. groups. Prediction (d) holds for all.

The poorer performance of the longer rest groups indicates that a depressing factor may be operating, perhaps the same as that influencing performance levels at the end of Period 1. Whatever is depressing performance in Period 3 has an effect about as great as that produced by allowing no rests at all. The 5-min. and 12-min. groups do not perform much better than the continuous practice group.

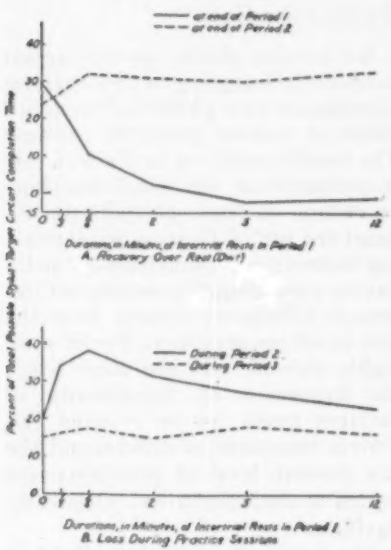

FIG. 3. Temporary work decrement, measured in terms of gain over rest and loss during a practice period, as related to initial distribution of practice

Temporary work decrement (D_{W_1}) as affected by initial distribution of practice

Fig. 3A shows D_{W_1} at the end of Periods 1 and 2 for the various distribution groups, measured as gain over the following rest. At the end of Period 1, D_{W_1} is increasingly less, the greater the distribution has been, up to 5-min. intertrial rests. This agrees with findings reported previously (2) for gains over a single rest following continuous practice.

By the end of Period 2, all groups should have developed the same amount of D_{W_1} since all had had continuous practice following a rest long enough to eliminate any effects from Period 1. Actually, the 0-sec. and 20-sec. groups developed less than the others. This superiority is not related to proficiency since the 50-sec. and 2-min. groups score higher than the 0-sec. and 20-sec. groups at the start of Period 3. Existing theory of temporary work decrement (1) needs to be amplified. Perhaps the more massed groups have acquired a greater resistance to the subsequent development of D_{W_1}.

Warm-up decrement (D_{wu}) as affected by initial distribution of practice

Fig. 4 shows D_{wu} at the start of Period 2 and Period 3. Initially in Period 2, there is progressively less D_{wu}, the longer the Period 1 intertrial rests. This bears out Prediction (e). By the start of Period 3, however, a change has taken place. There is still progressively less D_{wu} for the 0-sec., 20-sec., 50-sec., 2-min., and 5-min. groups, but the 12-min. group now shows more D_{wu} than the 50-sec., 2-min., and 5-min. groups. The 0-sec., 20-sec., 50-sec., and 2-min. groups show less D_{wu} than at the start of Period 2, whereas now the 5-min. and

12-min. groups show an appreciable amount.

It may be that some sort of expectancy affects D_{ww}, and that *Ss* are now responding to a new 12-min. practice period set as well as the old 20-sec. practice period set. The changes in D_{ww} from Period 2 to Period 3 do not parallel levels at the end of Period 1, losses during Periods 2 and 3, or levels at the start or finish of Periods 2 and 3. The changes in D_{ww} from Period 2 to Period 3 therefore are not associated directly with whatever is depressing the performance of the 5- and 12-min. groups.

Loss during practice periods as related to D_{WI}

In Period 1, final level of performance below optimum can be taken to indicate loss during the period. It has already been pointed out that loss parallels D_{WI} in this period for the 0-sec., 20-sec., and 50-sec. groups, but not for the 5-min. and 12-min. groups where performance is poorer than that of the 50-sec. group, and D_{WI} is much less. D_{WI} at the end of Period 2 and loss during Period 2 are shown in Figs. 3A and 3B. For the 0-sec., 20-sec., and 50-sec. groups, greater loss is accompanied by greater D_{WI}. This is not true of the 2-min., 5-min., and 12-min. groups, which show about the same amount of D_{WI} but increasingly smaller losses. Fig. 3B shows that losses during Period 3 are about the same for all groups.

Prediction (f) states that greater loss will be paralleled by greater D_{WI}. The prediction holds only for the shorter rest periods of 0, 20, and 50 sec. In the case of the longer rests, there is loss, but in Periods 1 and 2 it parallels performance level rather the D_{WI}. In Period 3, whatever has produced this effect disappears, and loss is not even related to performance level. Apparently loss during practice periods is not due solely to cumulation of D_{WI}.

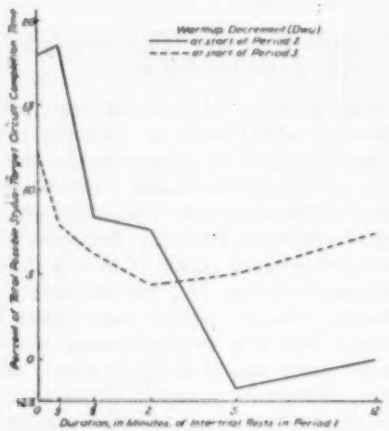

FIG. 4. Subsequent D_{ww} as a function of initial distribution of practice

Analysis of variance

To provide checks on the significance of the foregoing results, analyses of variance were performed on coded scores at various points in practice. The results are given in Table I. It is realized that the initial matching procedure at least partially invalidates the use of *F*-ratios, by decreasing between-groups differences without correspondingly decreasing within-groups differences. Scores from the first 20 sec. of practice in Period 1 are highly skewed. On the other hand, the hypothesis of homogeneity of variance could not be rejected (7). *F*'s are here used as indices, and the five percent level of confidence accepted as statistically and practically significant.

The first analysis given in Table I is of scores during the initial 20 sec. of practice in Period 1. The adequacy of matching is indicated by the rela-

TABLE I

RESULTS OF ANALYSIS OF VARIANCE AT VARIOUS POINTS IN PRACTICE

Sources of Variance Estimate: Coded* Scores from	Between-Groups Variance Estimates	Within-Groups Variance Estimates	p**
1. First 20 sec. of Period 1	9.40	28.7	Not meaningful
2. Last 60 sec. of Period 1	595	14.8	40.2
3. First 60 sec. of Period 2	223	17.3	12.9
4. Last 200 sec. of Period 2	40.2	14.5	2.77
5. First 60 sec. of Period 3	49.8	16.7	2.98
6. Last 200 sec. of Period 3	43.2	17.4	2.49

* The following coded scores were used for the indicated analyses: 1. raw scores in terms of .001-min., divided by 10; 2. raw scores in terms of .001-min., divided by 30; 3. same as 2.; 4. raw scores in terms of .001-min., divided by 100; 5. same as 2.; 6. same as 4.

** All between-groups variance estimates can be considered to have five d.f., and all within-groups variance estimates to have 138 d.f. An F based on five and 138 d.f. must be 2.28 or larger to be significant at the five percent level, and 3.15 or larger to be significant at the one percent level of confidence (7).

tively small size of the between-groups variance estimate. By the end of Period 1, differential distribution of practice has produced highly significant differences in performance levels. If there had been no warm-up decrement, the F's at the start of Periods 2 and 3 would probably have been about equal and comparable to those at the ends. Actually, there was much D_{ww} at the start of Period 2, but little at the start of Period 3, as indicated by the F's. The appearance of relatively permanent changes in performance level is indicated by the statistically significant F's at the ends of Periods 2 and 3. The sizes of the between-groups variance estimates are such that one might well conclude that the relatively permanent effects of distribution on performance level are much smaller than are the immediate effects or the effects on warm-up decrement.

These statistical findings agree with those from the other analyses of the data made up to this point.

DISCUSSION

The present findings with respect to relatively permanent decrement associated with different distributions of practice are of considerable theoretical importance. It was predicted (1) that permanent work decrement would develop in proportion to the amount and time present of temporary work decrement. This prediction only holds for the 0-sec., 20-sec., and 50-sec. groups in the present study. The 2-min. group is not superior in performance to the 50-sec. group, even though less D_{W1} is developed. Although they show no D_{W1}, the 5-min. and 12-min. groups are actually poorer than the 50-sec. group.

It woud seem that two factors may be operating, one leading to better performance with greater distribution up to 50 sec. or 2 min. between trials, and the other leading to poorer performance with greater distribution, especially beyond two min. between trials. The two factors would then work to produce the present curves showing performance levels corrected for D_{W1} and D_{ww} as best with 50 sec. or two min. between trials, i.e., with best performance (or least D_{W2}) at intermediate levels of distribution.

Perhaps the poorer showing of the 5-min. and 12-min. groups is partially due to the testing of performance level by

introducing continuous practice periods. It would be predicted from what is known about stimulus generalization gradients, that the longer the Period 1 intertrial rests, the more different the continuous practice would appear to *S*s, and consequently, the poorer the performance. Perhaps the failure of distributions greater than 50 sec. to lead to better performance in Period 1 is due to generalization or to interference. The longer the rest period, the more activities *S*s can engage in between trials. With more activities there could be more interference (8), and less phenomenal similarity of task from trial to trial, leading to less generalization and poorer performance. Motivation may also be lower with longer rests, since a longer stay in the experimental situation is required, and greater personal inconvenience results.

The effects of these variables could be explored fairly easily. Effects of distribution of practice during test periods could be determined by comparing results when half of each distribution group has a continuous-practice test period and half a relatively distributed-practice test period. Differential distribution could be maintained over a longer period to see if there really is an appreciable permanent work decrement. Motivation could be self- or judge-rated before and after practice to see if it is related to phenomena of distribution. Finally, different types and amounts of activities could be introduced during longer 'rest' periods of the same length to determine the importance of this variable in limiting the effectiveness of greater distribution.

Summary and Conclusions

Undergraduate women college students (N = 212) were given three 12-min. periods of rotary pursuit practice. In Period 1, *S*s rested 0 sec., 20 sec., 50 sec., 2 min., 5 min., 12 min., or 24 hrs. between 20-sec. trials. In Periods 2 and 3, the test periods, all *S*s practiced continuously to equalize temporary effects of differential distribution. Smoothed curves based on group mean performance levels and corrected for warm-up decrement were analyzed except in the case of the 24-hr. group. The following results were obtained and checked by analyses of variance:

1. During Period 1, groups with 0-sec., 20-sec., 50-sec., 2-min., and 5-min. intertrial rests developed progressively decreasing amounts of temporary work decrement. During Period 2, the 0-sec. and 20-sec. groups developed somewhat less than the other groups.

2. With temporary work decrement and warm-up decrement eliminated or controlled overall performance levels were best for the 50-sec. and 2-min. groups at the end of Period 1, the 50-sec. group at the start of Period 2, the 20-sec. group at the end of Period 2, and the 50-sec. group at the start and end of Period 3. Thus, the best performance was found at intermediate levels of distribution.

3. Warm-up decrement at the start of Period 2 was less, the greater the distribution had been during Period 1, with the 5-min. and 12-min. groups showing none. At the start of Period 3 the 0-sec., 20-sec., 50-sec., and 2-min. groups showed somewhat less warm-up decrement than in Period 2, while the 5-min. and 12-min. groups showed appreciably more.

4. Loss in performance level during Period 2 did not parallel amount of temporary work decrement except for the 0-, 20-, and 50-sec. groups. Losses during Periods 2 and 3 were not closely related to general performance level or to warm-up decrement.

The predictions made at the start of the paper are only partially supported by these results. Probably of most significance is the relatively poorer performance of the groups

given 5- and 12-min. intertrial rests as compared with those given somewhat shorter rests. Perhaps two variables are operating, one which depresses the performance of groups with shorter intertrial rests and one which depresses that of groups with relatively longer rests. This would account for better performance at intermediate distribution levels.

Motivation, 'testing,' and generalization theories of temporary and permanent work decrement were discussed, and several further experiments outlined.

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AN EXPERIMENTAL STUDY OF LEARNING UNDER AN IRRELEVANT NEED[1]

BY IRVING M. MALTZMAN[2]

State University of Iowa

INTRODUCTION

Theoretical problem

Recent discussions of the different viewpoints in learning (2, 10) place special emphasis upon the role of reinforcement in Hull's theory of behavior (3). One author states that, "the theory of reinforcement is in a somewhat unique status. It is so central that if it is not valid the system collapses" (2, p. 106). The second author asserts that, "it has become a primary postulate with whose correctness a complex structure of deductions and interpretations must stand or fall" (10, p. 533). Unfortunately, neither of the above authors has clearly demonstrated the manner in which the concept of reinforcement is related to the constructs, guessed-at-relations, and empirical laws constituting Hull's theory of behavior.

As presented by Hull in *Principles of behavior* (3), the intervening variable of learning ($_SH_R$) is explicitly defined by (1) the number of reinforcements (N), (2) magnitude of reward (W), (3) temporal delay of reinforcements (t), and in classical conditioning, (4) the interval between the conditioned and unconditioned stimulus. In so far as N is but one of several terms appearing in the definition of learning, it is not a member of a special class, a 'primary postulate,' but an empirical concept to be operationally defined. The controversy of a reinforcement theory of learning *vs.* a nonreinforcement theory therefore appears to center about the definition of the term N.

In Tolman's sign-gestalt theory, cognition or field-expectancy is a function of number of reinforcements (N) and other variables. N is defined by the operations of presentation of a stimulus-stimulus (sign-significate) sequence and the counting of such occasions. In Hull's reinforcement theory, learning ($_SH_R$) is likewise a function of N and other variables. N is here defined by the operations of presentation of a reinforcing or goal situation and the counting of such occasions.

If latent learning were to be demonstrated as an experimental fact, then either the empirical or analytic, or both of these components of Hull's postulate concerning the growth of habit strength might require modification in order to account for the experimental data (3, p. 178). Since the operations used to define the concept of magnitude of reward and delay of reinforcement entail the operations defining N, a change in the operational definitions of two other terms in the explicit definition of habit strength would be required. Whether or not the guessed-at functions relating the experimental variables and habit strength would have to be changed or their constants modified is an empirical question which can only be decided by experimental trial. Another alternative is the introduction of a new construct or some additional assumptions in order to incorporate the refractory data into the theory. The selection of either approach would be dictated by the facts and convenience.

However, it is not at all apparent that the demonstration of latent learning would drastically modify the remaining postulates of Hull's theory of be-

[1] This article is part of a dissertation presented in August 1948 to the faculty of the department of psychology of the State University of Iowa in partial fulfullment of the requirements for the M.A. degree. The writer wishes to express his appreciation to Professor Kenneth W. Spence for his many helpful criticisms and suggestions. The experimental results reported here were presented at the 1949 meeting of the Midwestern Psychological Association.

[2] Now at the University of California at Los Angeles.

havior. The postulates concerning stimulus trace and afferent neural interaction would remain unchanged. The postulate concerning unlearned receptor-effector connections would not require modification. Assumptions concerning stimulus generalization and effective habit strength would be affected only indirectly, since effective habit strength is defined in part by *N*. The assertion that effective habit strength is a function of the distance between the conditioned stimulus and the evocation stimulus would not be affected by a change in the definition of *N*. Postulate 7 concerned with reaction potential or the multiplicative relation between habit strength and drive would remain unchanged. Likewise assumptions concerning inhibitory potential, reactive inhibition and conditioned inhibition, would be unaffected by the findings of latent learning experiments. The same conclusion may be reached regarding the postulates of oscillation, and momentary effective reaction potential. None of these postulates appear in danger of serious modification as a result of the demonstration of latent learning. If this is the case, the assertions of some critics of Hull's theory of behavior that the system stands or falls on latent learning is largely gratuitous.

Previous experimental studies

Pertinent data on the problem of defining *N* are the experimental studies of latent learning employing a single choice point maze. Two procedures have been commonly employed in these studies. Animals motivated for a relevant goal object (food or water) have commerce with a goal object (water or food) for which they are satiated. Test trials are introduced by establishing a need or demand for the previously undesired goal object while satiating for the goal object relevant during training (1, 4, 6, 14, 16). In the second procedure, *S*s are satiated for the goal objects (food and water) for which they will be motivated during the later test trials while activated by a weak irrelevant need ('social drive,' or escape from confinement). Test trials are introduced by inducing a need for one of the previously undesired goal objects while satiating for the other (5, 9, 15). A third procedure has been used by Seward (11). Here *S*s deprived of food were allowed to explore a T-maze with the relevant goal object absent from both the discriminably different end boxes. *S*s were then placed directly into one of the end boxes and permitted to consume the reward now present. Test trials were introduced immediately following this single commerce with a goal object. *S*s placed at the start of the maze were allowed to run to one or the other end box. This procedure is similar to the 'reasoning' type of situation. Commerce with the maze must be combined with a commerce with the end box following a single encounter with a goal object found there.

Statement of problem

The present experiment is similar in design to the second type of experiment discussed. It is an attempt to determine whether *S*s unmotivated for given goal objects while activated by irrelevant needs are capable of learning the paths leading to the given goals as indicated by a significant increase in correct choices when the appropriate motivation is induced at a later time.

Experimental Procedure

Subjects

Thirty rats from the colony of the University of Iowa psychology department, 11 hooded and 4 albino females, and 10 hooded and 5 albino males, were used as subjects in the experiment. Their ages ranged from 80 to 150 days at the start of the preliminary training.

Apparatus

A single choice point T-maze was used; the floor plan is presented in Fig. 1. The walls and floor of the stem, arm, goal boxes and goal alleys were constructed of pinewood. Plywood was used to construct the social cages. The stem was 36 in. long and the arms 30 in. long. Both the stem and arms were 2½ in. wide and 4 in. high. Coarse hardware cloth covered the floor of the left goal box while a finer cloth covered the floor

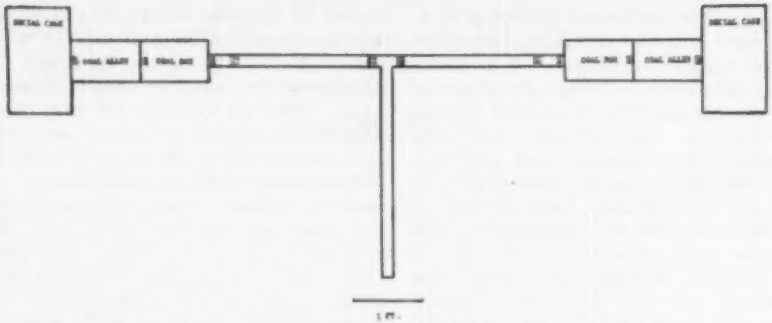

FIG. 1. Floor plan of apparatus. D_1, D_2, D_3, D_4, D_5, D_6—vertical sliding doors; D_7, D_8—one-way doors; C_1, C_2—curtains.

of the right goal box, providing for greater differentiability of the goal boxes. Vertical sliding doors controlled by the experimenter were located at both sides of the choice point and at the entrance of the goal boxes and goal alleys. A curtain was placed in front of each goal box in order to shield its contents from view of the *S*s. The sliding doors and the curtains originally were black in the right alley and white in the left alley. However, the doors and curtains on both sides were painted black after 20 *S*s had been run when a tendency to form position preferences towards black was observed. The social cages were 18 in. long, 12 in. wide, and 10 in. high, and were equipped with a one-way transparent celluloid door.

Overhead lighting was provided by a 100-watt bulb approximately eight feet above the choice point.

Satiation procedure

The satiation procedure employed during the preliminary and training periods was as follows: The *S*s were partially satiated for food by placing a large quantity of dog biscuits in their home cages two hours before they were to run in the experiment. One-half hour prior to running, the *S*s were placed in individual feeding cages containing dishes of wet mash in order to complete the satiation. Water was present in the home and feeding cages at all times.

The satiation procedure employed during the test trials for the hunger condition was as follows: One-half hour prior to running a test trial, *S*s under approximately 22 hours of food deprivation were placed in individual feeding cages containing a water bottle.

The satiation procedure used during the test trials for the thirst condition was as follows: One-half hour prior to running a test trial, *S*s under approximately 22 hours of water deprivation were placed in individual feeding cages containing eight gm. of dog chow.

The relative success of the satiation procedure used in the training period is indicated by the fact that approximately only 20 occasions of eating or drinking were observed out of a total of 1200 trials.

Preliminary training procedure

All *S*s were handled for at least 10 days preceding their preliminary training. During this time a drive rhythm based upon a diet of eight gm. of Purina dog chow per day was established. *S*s were adapted to the social cages by putting them into each cage for four-hour periods until they had spent a total of 24 hours in each social cage.

There were two days of preliminary training, two trials per day, in which the satiated *S*s ran down a straight runway into one or the other of the social cages containing a cage mate which otherwise did not participate in the experiment.

Cage mates were randomly assigned to the social cages each day during all phases of the experiment.

Training procedure

Five males or five females were run as a group. The males completed their trials before the females were run. Two trials per day, the first free and the second forced to the opposite side, were given for a period of 20 days. Perception of the goal objects was insured by placing a barrier of food in the left goal box which forced the *S*s to push the biscuits aside or step over them in order to pass into the alley leading to a social cage. A water dish three in. in diameter placed on the floor of the right goal box served a similar function by forcing the *S*s to walk through the water, as many did, or skirt the dish in order to pass into the social cage.

Every three min. an *S* in a running group was removed from its feeding cage and placed in a confinement cage. The *S* remained in the confinement cage for 15 min. prior to the first trial and was returned to it immediately after the trial. Each *S* was allowed to remain in the social cage for two min. after entering from the goal alley. Approximately 15 min. elapsed between the first and second trial of each *S*. Immediately after the second, or forced trial, the *S*s were returned to their home cages.

Test trial procedure

Test trials began the day following the 40th trial. *S*s deprived of either food or water for approximately 22 hours while satiated for the other goal object were given one test trial on each of four successive days. The drive conditions were alternated in the order, THHT or HTTH, depending upon an initial assignment. The *S*s were permitted to remain 20 sec. in the goal box or social cage of the alley chosen. They were then returned to the confinement cages for 10 min. before being placed in the home cages. One hour later they were given their food and water rations.

Results

Results of the four test trials in terms of the percent of correct responses are shown in Table I.

TABLE I

Percent of Correct Responses on All Four Test Trials

Test Trial	Percent Correct
1	56.7
2	63.3
3	60.0
4	50.0
Mean	57.5

A graphic comparison of the percent of training responses made to the side containing the goal object appropriate to the drive condition of the first test trial, and the percent correct during the first test, is shown in Fig. 2.

Two tests of significance were applied to these results. The first test was to determine whether the frequency of correct response on the first test trial differed significantly from chance. It resulted in a chi-square of .53 which is significant at only approximately the 48 percent level of confidence. A standard error of related proportions (7) was calculated to determine whether performance on the first test trial differed significantly from performance on the last day of training.

The proportion of appropriate re-

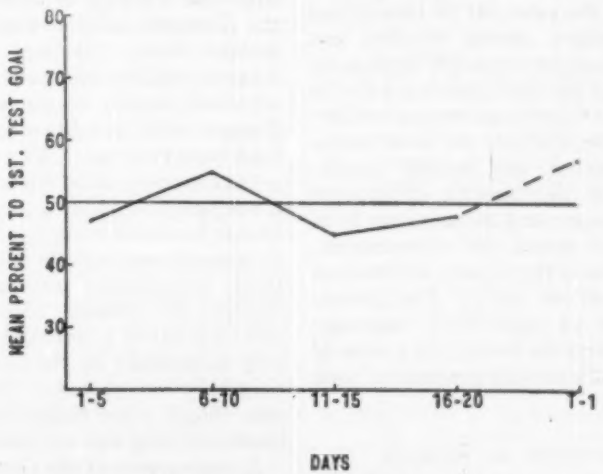

Fig. 2. Percent responses during training and first test trial to the alley containing the goal object appropriate to the drive condition of the first test trial

TABLE II

PERCENT OF RESPONSES TO APPROPRIATE GOAL MADE BY *Ss* HUNGRY AND BY *Ss* THIRSTY DURING FIRST TEST TRIAL AND LAST TRAINING TRIAL

	Percent of Appropriate R's on Last Training Trial	Percent of Appropriate R's on First Test Trial
Hungry	20.0	33.3
Thirsty	66.7	80.0

sponses on the last training day was .434, on the first test trial .567. A standard error of .259 was obtained, which is only .52 as large as the obtained difference. A critical ratio of this size is significant at only approximately the 60 percent level of confidence.

Table II gives the percent of *Ss* hungry and of *Ss* thirsty during the first test trial that chose the alley leading to the appropriate goal on their first test trial and last free training trial (satiated).

The difference between the percent of appropriate responses on the last training trial and the first test trial, 13.3, was the same for *Ss* thirsty and for *Ss* hungry during the first test trial. These results would seem to indicate that the need states induced in the present experiment were not differential in their effects on latent learning. However, any general conclusion about the possible differential role of hunger and thirst needs from these data would be unwarranted, since the need states were confounded with a position habit. The greater percentage of appropriate responses by the thirsty *Ss* during the course of training indicates the presence of such a habit.

DISCUSSION OF RESULTS

It would appear from the results reported that the present experiment does not sustain the delimitation of the experimental variable *N* to the operations of the presentation of a stimulus-stimulus sequence and a counting of such occasions as proposed by the sign-gestalt theory. The problem remains, however, of incorporating into a reinforcement theory of learning the contradictory results of the relevant experiments.

The writer has attempted an explanation of these data on the basis of Spence's analysis of stimulus generalization and secondary reinforcement (12), and his formulation of the fractional anticipatory response (13).

So-called latent learning as defined by the general experimental procedures discussed earlier is proposed to be a function of the competition of fractional anticipatory responses for the relevant and irrelevant goal objects. One of the variables determining the strength of the anticipatory responses, and hence the amount of latent learning evidenced, is the temporal delay between the choice point and the goal objects. It is believed that this variable is responsible for the divergent results obtained in the present study. It was previously pointed out that the social cages were attached directly to the arms of the T-maze while in other studies of this kind they were not. This shorter delay in obtaining social reward produced a stronger fractional anticipatory response for social reward, permitting it to compete successfully with the fractional anticipatory responses for the relevant goal objects. Since the correct response in a test trial is presumably determined by the fractional anticipatory response for the relevant goal object, other things being equal, latent learning was not evidenced.

A consequence of the above analysis is the derivation of apparently contradictory data from a common set of

principles and assumptions. That the formulation outlined here is more than a simple *ad hoc* construction is indicated by the derivation of a number of testable implications (8).

Summary

An attempt was made to delimit the status of reinforcement in a theory of behavior. It seemed that the controversy of reinforcement *vs.* nonreinforcement theories of learning centered about the definition of one of the experimental variables (N) determining the hypothetical construct of learning. A review of the relevant experiments was given, and this experiment was presented as an additional investigation of the problem.

Thirty rats satiated for food and water were given two trials per day for 20 days in a T-maze with food on one side and water on the other. The *Ss* were motivated to run by a 'social need.' A test period that followed consisted of one trial per day on four successive days in which the *Ss* were motivated for one of the goal objects while satiated for the other. It was found that behavior on the critical first test trial did not differ significantly from chance; nor did it differ significantly from the last training trials. The sign-gestalt view of learning as a function of the number of stimulus-stimulus sequences therefore was not confirmed.

A theoretical formulation employing the construct of the fractional anticipatory response, and the principles of stimulus generalization, secondary reinforcement, and an analysis of the important temporal variables was proposed to account for the different experimental results dealing with the problem.

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December 10, 1949)

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THE EFFECT OF INTERMITTENT REINFORCEMENT ON THE CHANGE IN EXTINCTION RATE FOLLOWING SUCCESSIVE RECONDITIONINGS [1]

CHARLES C. PERKINS, JR., AND ANTHONY J. CACIOPPO [2]

Kent State University

Introduction

Recently there has been considerable interest in studies involving reinforcement on a given percent of the learning trials, rather than on every trial in the learning series. This interest stems largely from a series of studies by Humphreys (5, 6, 7) in which he found that intermittent (50 percent) reinforcement (a) led to the same level of performance as that obtained when every trial was reinforced, and (b) established a response which was *more* resistant to extinction than if continuous reinforcement had been used. Humphreys (5) and Hilgard (2) have interpreted these findings as contrary to a stimulus-response reinforcement theory of learning such as Hull's (4). However, two recent studies in which white rats were the subjects have shown that rather than contradicting Hull's theory, the results of intermittent reinforcement studies can be very nicely integrated with an S-R reinforcement theory.

The first of these studies is a well conceived experiment by Denny (1) which demonstrated rather clearly that at least under some conditions learning is as rapid (in terms of number of trials) under conditions of intermittent as 100 percent reinforcement only if there is actually secondary reinforcement on the 'non-reinforced' trials. Thus, Hull's assumption that level of learning is a function of the number of *reinforcements* is not inconsistent with Humphreys' first finding (*a* above).

In the second study Sheffield (12) tested a deduction from Hull's theoretical explanation (3) of the great resistance to extinction shown by responses set up by intermittent reinforcement. This explanation emphasizes that in the typical learning or conditioning situation the compound conditioned stimulus during training has as one component the after-effects of the preceding reinforced trial. On all extinction trials except the first this component is not present, but is replaced by the after-effects of a non-reinforced trial. Extinction is facilitated by this difference between the conditioned stimuli on the two series. Thus, extinction is actually a test of stimulus generalization, an example of 'transfer of training.'

Following intermittent reinforcement (during which the majority of the reinforced trials follow a non-reinforcement) there are no marked differential cues distinguishing extinction from learning trials. Thus, with near identity of stimuli during learning and extinction series, transfer appears to be practically complete and consequently extinction is unusually slow. However, much less than 100 percent transfer from conditioning to extinction follows continuous reinforcement where the difference between the stimulus complexes is marked.

Sheffield reasoned that since the after-effects of a trial could be expected to dissipate with time, there would be an interaction between spacing of training trials and the type of reinforcement (continuous or intermittent). That is, type of reinforcement should have less effect with greater spacing of trials because this produces a decrease in the importance of

[1] This study was done as a Master's thesis problem at Kent State University by one of the authors (A. J. C.). The other (C. C. P.), who was the thesis advisor, is responsible for the write-up in its present form.

[2] Now at the State University of Iowa.

the after-effects of the preceding reinforcement or non-reinforcement. Sheffield found such an interaction between the effects of spacing of trials and type of reinforcement on extinction rate of a simple running response.

The fact that the effects of intermittent reinforcement can be explained by S-R theory does not mean that these findings have become less significant for such a theory. So far as the present writers can see, only this frame of reference would lead to a clear-cut prediction of Denny's and Sheffield's results. Other theories as so far developed would not lead to any particular prediction.

Further tests of deductions from reinforcement theory are now of even greater importance. There is also considerable need for further studies to determine the full implications of the difference between the compound stimulus presented during conditioning and extinction. It may well be that this factor must be considered in any theory of extinction and that it may serve to integrate findings which have not been previously related.[3]

Several writers (9, 11, 12) have suggested that extinction is more rapid following continuous reinforcement because of the facilitative effect of discrimination between conditioning and extinction series. This facilitative factor might well be partially responsible for more rapid rate of extinction with each successive extinction series following conditioning or reconditioning. Such an increase in extinction rate has been reported by Pavlov (10) and Skinner (13).

The increase in extinction rate which has been reported with successive extinction series should be less rapid with intermittent than continuous reinforcement during conditioning and reconditioning series, since with continuous reinforcement, a discrimination between conditioning and extinction trials can be more easily established.

The experiment here reported is designed to determine if this hypothesized interaction between condition of reinforcement and change in extinction rate with successive series holds. Verification of the deduction may be taken as evidence that the change in rate of extinction with successive extinction series is a special case of discrimination learning.

[3] A more complete theoretical analysis of this problem is presented elsewhere (11).

Procedure

Apparatus.—A modified Skinner-box or bar-depression apparatus was used in this experiment. The interior of the box formed a cube one foot on a side. The box was constructed from one-half in. plywood throughout, with the exception of an 8 × 8 in. glass observation window in the center of the roof of the apparatus. Ventilation of the box was effected by raising one edge of the glass window one-half in. above the roof of the box. The apparatus was not sound proofed, but it was placed in a room which was relatively free of distracting noises. Observation was facilitated by mounting a mirror at a 45° angle above the window at the top of the apparatus.

The bar, which was shaped like a T, projected horizontally from the wall of the apparatus, the one-in. cross-piece being horizontal and parallel to the wall. By sliding the bar mechanism two in., *E* placed the bar in the response position. When in this position the bar projected 1½ in. through the bar slot. As the bar was moved into the response position, a click sounded and the circuit controlling the recording clock was closed, automatically starting the clock. Upon depression of the bar (about five-tenths cm. with a pressure of approximately four gm.) the latency recording clock stopped. The bar slot was located on the back of the box, 3½ in. above the floor board and midway between the two sides. The tin food tray was fixed at a point 2½ in. from the left wall with the glass food dispenser tube projecting into the tray. The tube was mounted flush with the outer side of the back of the box, with an inch of it projecting into the box above the food tray. A flashlight bulb was located at the top of the box immediately above the bar. The bulb was so wired that it could be cut out of the circuit by the experimenter. When the bulb was lit it clearly increased the lighting of the interior of the apparatus as judged by human observers.

Subjects.—The subjects were 22 female albino rats from the colony maintained by the Kent State University Psychology Department. At the beginning of training the mean age of the

TABLE I

SUMMARY OF THE EXPERIMENTAL DESIGN

Days	Type of Reinforcement: 100% Reinforcement	50% Reinforcement
1–4	10 light-food-click 5 click alone	10 light-food-click 5 click alone
5–6	16 reinforced trials	16 reinforced trials
7–9	16 reinforced trials	16 trials, 8 reinforcements
10–17	As days 7–9, plus 30 min. extinction	As days 7–9, plus 30 min. extinction

animals was 101 days; the range was from 60 to 120 days. Since one animal lost a tooth during the course of the experiment, this subject and its control were discarded.

Experimental procedure.—The animals were run in squads of two, one for each of the two experimental conditions, i.e., 100 percent and 50 percent reinforcement. Each squad consisted of litter mates. The paired animals never differed by more than 10 gm. in weight. At least three days before the beginning of preliminary training *Ss* were placed on a 24-hour feeding schedule which was maintained throughout the course of the experiment. For equation of the hunger drive, the weight of the *Ss* was gradually reduced to 90 percent of normal body weight. Throughout the experiment, daily feedings which followed the experimental period were just sufficient to bring the body weight up to 90 percent. Purina Fox Chow was used for regular feedings, and for reinforcement.

A reinforced trial consisted of a bar-pressing response immediately followed by lighting of the flashlight bulb and the presentation of a two-tenths gm. food pellet. The light stimulus was turned off by *E* two sec. after the response was made. The non-reinforced trial consisted of a bar-pressing response which was followed by neither light nor food. The bar was withdrawn as rapidly as possible after every trial.

All the animals received the same training for the first four days. During this period each animal was placed in the apparatus with the bar withdrawn. The animal was then presented with 10 light-click combinations each followed by a two-tenths gm. pellet of food. These combinations were presented at 30-sec. intervals. After a series of 10 paired presentations, five relay clicks were sounded alone at 30-sec. intervals.[4]

On days five and six, each group received the same training, which consisted of 16 bar presentations, all of which were reinforced.[5] Food paste was smeared on the bar for the first two trials, but after the second trial the bar was washed with water to clean off all traces of food paste. On every trial during these days and throughout the rest of the experiment, the bar was presented thirty seconds after the preceding response.

Differential training for the two groups was started on the seventh day. On days seven through nine, the 100-percent group received sixteen reinforced trials and the 50-percent group received eight reinforced and eight non-reinforced trials. The sequence of reinforced and non-reinforced trials was predetermined so that the order was irregular and there were no sequences of more than two successive reinforced or non-reinforced trials.

From the 10th to the 17th day of this study, the experimental session was divided into two phases: conditioning and extinction. The learning phase was the same as on days seven to nine. The daily extinction period lasted 30 min. A summary of the experimental design is outlined in Table I.

RESULTS AND DISCUSSION

No detailed analysis of the conditioning or reconditioning has been made, since our interest is primarily in extinction. However, qualitative observations of *E* indicated that the animals were responding at their maximum rate by the end of day six, before differential training was started. Thus, the first extinction followed considerable 'overlearning.' It is also of interest that so far as could be observed, spontaneous recovery was

[4] These clicks were presented alone to extinguish any possible secondary reinforcing properties of the clicks. There was no evidence that click alone had any secondary reinforcing properties.

[5] This procedure was used because exploratory work had indicated that establishment of the response with 50-percent reinforcement (and no secondary reinforcement on 'nonreinforced trials') was slow. This observation suggests that Denny's findings would hold for establishment of a bar depressing response.

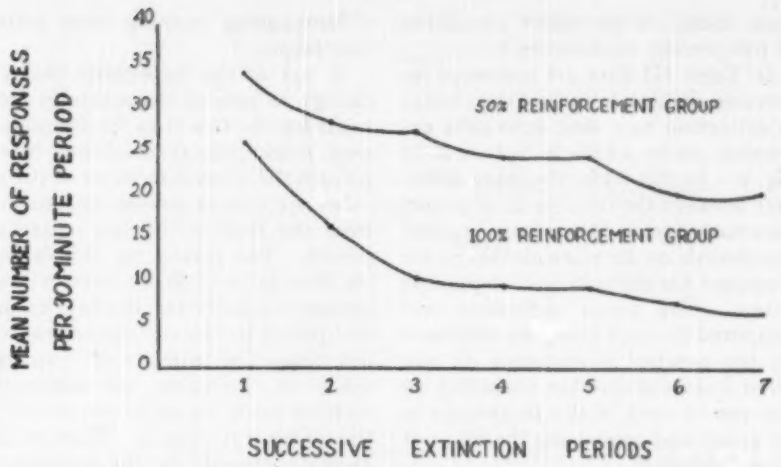

FIG. 1. Curves showing the relationship between the mean number of responses during a 30-min. extinction period and the ordinal number of the extinction series. The upper curve is based on the 10 *Ss* in the 50-percent reinforcement group, the lower on the 10 animals trained only by continuous reinforcement.

complete before each reconditioning series.

In Fig. 1 and Table II evidence is presented which is in line with the findings of Humphreys (5, 6) and Mowrer and Jones (9) that extinction is slower following intermittent reinforcement on training trials. The mean number of responses made by the 50- and 100-percent groups during daily 30-min. extinction periods are shown there. As we compare the 50- with the 100-percent groups, we see that the mean number of responses is greater under the former condition for equivalent extinction periods.

As shown in Table II, the differences between mean number of responses made by the groups during corresponding extinction periods is significant at the 0.1 percent level of confidence, except for the second extinction where the difference is significant at the one percent level of confidence. Thus, under the particular conditions of this experiment, habits set up under conditions of 50-percent reinforcement are more resistant to extinction

TABLE II

MEAN NUMBER OF RESPONSES DURING EACH EXTINCTION PERIOD FOR BOTH GROUPS, THE DIFFERENCES BETWEEN THE TWO GROUPS FOR CORRESPONDING PERIODS AND THE SIGNIFICANCE OF THESE DIFFERENCES

	1	2	3	4	5	6	7
$M_{100\%}$	26.1	16.8	10.6	9.6	9.4	7.6	6.5
$M_{50\%}$	34.1	28.7	27.5	24.6	24.7	20.3	20.5
$M_{50\%}-M_{100\%}$	8.0	11.9	16.9	15.0	15.3	12.7	14.0
t	8.05	4.37	7.81	9.75	6.72	4.82	4.90
P	<.001	<.01	<.001	<.001	<.001	<.001	<.001

than those set up under conditions of 100-percent reinforcement.

In Table III data are presented indicating the significance of the change in extinction rate with successive extinction series which is apparent in Fig. 1. In this table, the mean difference between the number of responses occurring during the 30-min. extinction periods on days one and seven are presented for the 100- and 50-percent groups. The mean difference was computed for each group by subtracting the number of responses on day seven from the number occurring on day one for each of the 10 animals in the group and computing the mean of these differences.

TABLE III

MEAN DIFFERENCE BETWEEN THE NUMBER OF RESPONSES MADE DURING THE 30-MINUTE EXTINCTION PERIOD ON DAY ONE AND DAY SEVEN FOR 50% AND 100% GROUPS

Group	N	Mean Difference	σ Difference	σ_M Difference	t	P
100%	10	18.6	6.2	2.07	9.01	.001
50%	10	13.6	12.6	4.20	3.23	.01

The exact hypothesis that the mean of these differences is zero was tested for both groups by means of the t-test. As shown in Table III, this hypothesis can be rejected at the 0.1 and one percent levels of confidence for the 100- and 50-percent groups, respectively. This statistical test taken in conjunction with the clear trends which can be seen in Fig. 2 indicates clearly that there is a decrease in number of extinction trials with successive extinction series. According to the hypothesis that any increase in extinction rate is dependent upon a discrimination between learning and extinction, these results imply that under both conditions there are cues differentiating learning from extinction trials.

A test of the hypothesis that the change in rate of extinction is more rapid for the 100- than for the 50-percent reinforcement condition is not meaningful if made in terms of the *absolute* decrease in number of responses from the first to the last extinction period. The reason for this is that for three pairs of *S*s the number of responses made during the first extinction period by the 100-percent rat was less than the number of responses made on extinction one minus the number made on extinction seven by the 50-percent animal. That is, the absolute decrease for the 100-percent group sometimes could not be as great as for the 50-percent group due to the difference in extinction rates on day one.

Therefore, the number of responses in the seventh extinction period divided by the number made during the first extinction was used as an index of change in the rate of extinction. For example, the first *S* in the 100-percent group made 26 responses during his first extinction and six during the last. The index for this rat is 6/26 or 0.231. This index was computed for each subject. Then the index for the 50-percent *S* minus the index for the corresponding 100-percent animal was determined for each pair of subjects. Then, since the 50-percent animal in the first pair had an index of 14/22 or 0.636 the difference between the indices for these two *S*s is 0.405 (0.636–0.231). The exact hypothesis that the mean (0.356) of these differences is zero was then tested by the t test.[6]

[6] The formula used for the test of significance was

$$t = \frac{M_o - M_h}{\frac{d^2}{n(n-1)}}.$$

This method of testing the null hypothesis for re-

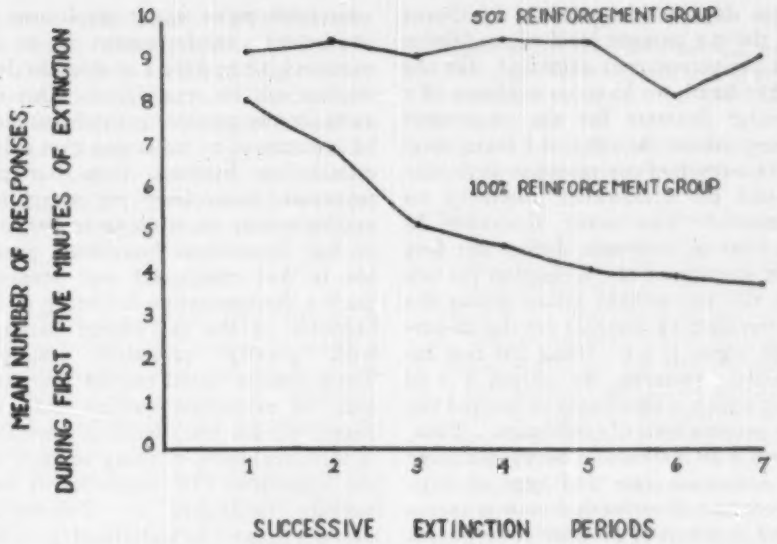

FIG. 2. Curves showing the relation between the mean number of responses during the first five min. of the extinction period and the ordinal number of the extinction series. The upper curve shows this relationship for the 50-percent group; the lower for the 100-percent *Ss*.

A t of 3.60 was obtained. Thus, with nine degrees of freedom, we may reject the hypothesis that the index is the same with both conditions of reinforcement at the one percent level of confidence. We may now conclude that change in extinction rate as measured by our index is greater for continuous than for intermittent reinforcement.[7] Thus, our results are in line with the hypothesis the experiment was designed to test.

TABLE IV

MEAN NUMBER OF RESPONSES MADE BY EACH GROUP DURING FIRST FIVE MINUTES OF EXTINCTION ON DAYS ONE AND SEVEN, MEAN DIFFERENCE BETWEEN THE NUMBER OF RESPONSES ON THESE DAYS AND THE SIGNIFICANCE OF THIS DIFFERENCE

Group	N	$M_{day\ 1}$	$M_{day\ 7}$	M_{1-7}	t	P
100%	10	8.2	3.9	4.3	5.81	.001
50%	10	9.3	9.2	0.1	0.17	—

The extinction data based on the first five min. of each extinction are presented in Fig. 2, since the beginning of the extinction periods is of particular theoretical significance. In this graph, the mean number of responses during the first five min. of extinction is plotted against the ordinal number of the extinction series for each group. As shown in Table IV, the decrease in number of responses during the first five min. of extinction

lated measures is outlined by Lindquist (8, p. 58). The same method is used to test for the significance of the difference between the means of related samples for all such tests below.

[7] Further evidence of the significance of the difference between the change in number of responses during extinction for the two groups comes from the fact that for only one of the 10 pairs of animals was the index larger for the member of the pair with 100-percent reinforcement. From expansion of the binomial we find that as many as nine of the ten differences would be in this direction only 11 times per 1024.

from days one to seven is significant at the 0.1 percent level of confidence for the 100-percent animals.[8] On the other hand, we have no evidence of a similar decrease for the 50-percent group where the obtained mean drop of one-tenth of one response obviously could be attributed entirely to chance.[9] The mean decrease in number of responses during the first five minutes of the extinction periods for the 100-percent group minus the corresponding decrease for the 50-percent group is 4.2 Using the test for related measures, we obtain a t of 6.03 which is significant at beyond the 0.1 percent level of confidence. Thus, there is an interaction between change of extinction rate and type of reinforcement if extinction rate is measured in terms of number of responses during the first five min. of extinction. This confirms our conclusion from the results when responses during the entire extinction period is the measure used.

Mowrer and Jones (9) have reported that "rats can learn to discriminate between one and two bar depressions and between two and three, but beyond this their discrimination becomes unreliable" (p. 296). Thus, our finding that extinction rate during the first five minutes does increase with successive series under conditions of 100-percent reinforcement is to be expected, since here a stable discrimination can be established. Our results for the 50-percent condition may be interpreted as indicating that a discrimination between three bar depressions (sometimes necessary for reinforcement on training trials) and 10 bar depressions (maximum possible in five min.) did not develop. Such a discrimination is hardly to be expected in the rat except perhaps with greatly extended training. These results based on the first five min. of extinction (which unfortunately we did not specifically predict in advance) offer striking support of the hypothesis that extinction is frequently facilitated by differential cues not present on conditioning trials.

The results reported above support fully the hypothesis that: (1) there are differential cues on conditioning and extinction series which facilitate extinction; (2) that this facilitative effect is more marked with 100- than with 50-percent reinforcement; and (3) that the effect becomes greater with further conditioning and extinction series for the same subject. Although some of the conditions of the experiment probably produced especially marked after-effects, e.g., the reinforcement with both light and food, other conditions, e.g., 30-sec. spacing of trials, are not especially conducive to development of a clear cut discrimination on the basis of these after-effects. It seems to us that this factor of differentiation based on the after-effects of preceding trials is of considerable importance in many learning situations and must become an integral part of any theory of extinction. This point has been developed further elsewhere (11).

In our opinion, other explanations of the greater resistance to extinction

[8] The same test of significance was used here as for the corresponding test for the entire extinction period.

[9] A decrease in number of responses from the first to the last extinction period during the first five min. of extinction was obtained for nine animals while there was no change for the tenth in the 100-percent condition. There were four decreases and two increases in the 50-percent group, the other four *S*s showing no change.

It is entirely possible (though in view of other findings it seems unlikely) that there was no evidence of a change in extinction rate because the response was overlearned so much that a great change in the strength of the habit would be necessary to allow it to appear, since even with a marked drop in response tendency the animals would still respond at the maximum rate possible.

shown by responses set up by intermittent reinforcement, especially Humphreys' loosely formulated 'expectancy hypothesis' (5), cannot be used to predict our results. Perhaps Hilgard's charge (2, pp. 112–113) that Hull's system neglects experiments on intermittent reinforcement would be better directed against any other learning theory.

Summary

1. Two groups of 10 white rats each were strongly conditioned to make a bar-depressing response, one under conditions of continuous reinforcement and the other by 50-percent reinforcement. Both groups were then extinguished, reconditioned, re-extinguished, etc., until there had been a total of seven extinction periods.

2. Animals trained under 50-percent reinforcement were found to be more resistant to extinction on comparable extinction series than those trained under 100-percent reinforcement.

3. Both groups showed fewer trials to extinction with each successive extinction series.

4. The change in rate of extinction with successive extinction series was significantly more rapid under conditions of continuous reinforcement.

5. If only the first five min. of the extinction periods are analyzed, no change in rate of extinction is found for the 50-percent group. However, a change in rate was found to be statistically significant for the 100-percent group.

6. These results are in complete agreement with predictions from an extension of Hull's theory.

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THE INTER-RELATIONSHIP OF OPERANT LEVEL, EXTINCTION RATIO, AND RESERVE

BY DONALD H. BULLOCK

University of Buffalo

Three important measures of operant behavior are: (1) the operant level, (2) the extinction ratio, and (3) the reserve. The following study reports on the inter-relationship of these three measures in the Skinner box.

1. Operant level: the pre- or unconditioned rate of response of an operant response is referred to as the operant level or operant rate (5). The measurement of this pre-conditioning strength is usually done by means of one or more 'free activity' sessions, in which the response (lever-pressing) is allowed to occur but no experimentally-produced reinforcement is provided. The number of responses per session typically declines (5), so that it is generally advisable to obtain a series of measurements. Previous data (3, 5), as well as that obtained in this research, indicates that in four to six days the operant level has reached a stable value, the actual value depending on such conditions as the drive (5), etc. In this experiment, five consecutive daily half-hour sessions were conducted. The measure of the operant level for any given rat was the median number of responses per 30 min. for the five sessions.

2. Extinction ratio: during periodic reinforcement, in which responses are reinforced at regular temporal intervals, the number of responses occurring between successive reinforcements tends to reach a stable value (6). This value is said by Skinner to represent the height of the reserve (see [3] below). This being the case, a high positive correlation should be found between the number of responses per reinforcement interval during conditioning with periodic reinforcment (called by Skinner the 'extinction ratio') and the resistance to extinction of the response (called by Skinner 'reserve') during subsequent extinction. In this experiment the animals were given a total of 60 reinforcements, during two successive daily sessions, at an interval of two min. The measure of the extinction ratio for a given animal was the median number of responses per reinforcement interval during the second periodic conditioning session (30 reinforcements).

3. The reserve: Skinner has applied the term 'reserve' to the total number of a specified response available in an organism's repertoire. Typically, psychologists have referred to the term as "resistance to extinction," since a large part of the reserve is the potential number of extinction responses added by reinforcement. However, one important difference has resulted from use of the term reserve. That is, 'Skinnerians' have usually obtained prolonged extinction curves, ranging from one to eight or more hours in duration, whether obtained at one session or in successive sessions. 'Non-Skinnerians' have, on the other hand, been more inclined to measure the number of responses required to attain a criterion, such as five min. of no-response. The objection to this latter precedure is (a) that data reported by the Indiana Laboratories (4) indicates little correlation between this criterion and the total number of responses obtained in six hours of extinction; and (b) if some variable, such as the duration of the interval separating extinction trials or sessions, operates in such a manner as to initially increase the rate of response without materially affecting the overall total number of responses (1), then this would likely not be shown in research utilizing a five-min. criterion.

In this experiment the reserve was measured by allowing the conditioned animals to respond to the lever without experimentally administered reinforcement for a five-hour continuous session.

The measure of the reserve was simply the total number of lever-pressings occurring during this five-hour extinction.

Since the operant level stabilizes at a point above zero (if it did not, conditioning could not occur), this operant level may well be reflected in subsequent extinctions, thereby influencing the reserve obtained. One aim of this experiment was to correlate pre-conditioning operant level with the post-conditioning reserve.

The Problem

To determine the product-moment inter-correlations of the following aspects of operant behavior: pre-conditioning operant level; the extinction ratio obtained during periodic reinforcement conditioning; and the reserve or resistance to extinction following this conditioning.

The following specific problems were under consideration:

1. Is the pre-conditioning rate or strength of response reflected in (a) the extinction ratio during PR, or (b) the reserve?

2. Is the extinction ratio positively correlated with the reserve, as Skinner's formulation would demand?

Apparatus and Procedure

The basic appratus used has been described in detail elsewhere (2). The living-experimental cage, lever, feeder, and four-channel polygraph of the Columbia University laboratory were utilized.

1. Operant level: following the usual preliminary procedure of habituation to the living-experimental box and the establishment of an eating schedule, the rats were each given five half-hour sessions on consecutive days during which the lever was available but no responses were experimentally reinforced.

2. Extinction ratio: following the operant level sessions, conditioning by means of regular reinforcement with food (finely ground Purina laboratory chow, each reinforcement consisting of approximately 1/20 gm.) was conducted. The lever was inserted, 30 consecutive responses were reinforced, the first and 30th with quintuple-reinforcement (5/20 gm.), and the lever was retracted. Two hours later, 30 reinforcements were administered at two-min. intervals. The following day this procedure was repeated, with omission of the original 30 regular reinforcements.

3. Extinction: the day following the second PR training session, the lever was inserted and remained available continuously for five hours. No reinforcement was experimentally administered.

With the exception of the first PR session, the rats had been deprived of food for approximately 20 hours prior to the start of an experimental period. The animals, male albinos from the Wistar Colony, were about 130–140 days of age at the start of experimentation. All of the 32 rats had been previously used in an experiment on the magnitude of reinforcement in the Graham-Gagné runway (7). No relationships were found between differences in their prior history and their lever-pressing behavior.

Results

Table I presents the product-moment correlations[1] of the three measures used.

TABLE I

The Correlations between the Operant Level, Extinction Ratio, and Reserve, and the Multiple Correlation of the Operant Level–Extinction Ratio and the Reserve

Operant rate and reserve	.474 ±.094*
Extinction ratio and reserve	.611 ±.076*
Operant rate and extinction ratio	.173 ±.085
Operant rate-extinction ratio and reserve (multiple correlation)	.716*

* Statistically significant.

1. Operant level and extinction ratio: the product-moment correlation of these two measures (.17) shows a statistically insignificant relation. Evidently the operant level is not appreciably reflected in the rate of response during periodic reinforcement conditioning.

2. Operant level and reserve: the product-moment correlation of these

[1] Research conducted in the Columbia University Psychology Laboratory, 1947. Statistical computations by Mr. Louis J. Gerstman.

two measures (.47) shows a statistically significant positive relation. The operant rate exerts an appreciable influence in the reserve or resistance to extinction.

3. Extinction ratio and reserve: the product-moment correlation of these two measures (.61) shows a statistically significant positive relation. Skinner's formulation, which holds that the extinction ratio—the number of responses per reinforcement interval during PR training—is a reflection of the size of the reserve, is substantiated by this correlation.

4. Operant level and extinction ratio (combined) and the reserve: the multiple product-moment correlation of these measures (.71) shows that by combining the operant level and extinction ratio measures we can raise the predictive value of our correlation with regard to the reserve (from .61 to .71 for the extinction ratio alone; from .47 to .71 for the operant level alone).

Since the operant level is significantly reflected during extinction, it would seem desirable to equate groups of animals on the basis of operant level prior to experimentation proper in which reserve or resistance to extinction is to be used. Such equating would heighten both the reliability and validity of experimental findings.

Since the operant level is not appreciably reflected in the extinction ratio, the possibility might well be considered that the extinction ratio is actually a more adequate measure of the *reserve produced by reinforcement* than is the reserve as measured during extinction.

Summary and Conclusions

Thirty-two rats were used in an experiment in which five daily operant level sessions, followed by one regular reinforcement and two periodic reinforcement conditioning sessions, and a subsequent five-hour continuous extinction session were conducted in the Skinner box. The inter-correlations of three measures of the lever-pressing behavior were computed. They showed:

1. The operant level is significantly correlated with reserve.
2. The operant rate is insignificantly correlated with the extinction ratio.
3. The extinction ratio is significantly correlated with the reserve.
4. Combining of the operant level and extinction ratio increases the correlation of these measures with the reserve.

(Manuscript received
December 10, 1949)

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The American Psychological Association, founded in 1892 and incorporated in 1925, is the major psychological organization in the United States. With approximately 6,700 members and 2,000 affiliates, it includes most of the qualified psychologists in the country. The purpose of the APA is to advance psychology as a science, as a profession, and as a means of promoting human welfare. It attempts to further these objectives by holding annual meetings, publishing psychological journals, conducting a personnel placement service for psychologists, and working toward improved standards for psychological training and service.

In order to give recognition to the specialized interests of different psychologists, the APA includes seventeen Divisions. Any person, after becoming a member of the APA, may apply for membership in as many Divisions as he wishes. The Divisions are:

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The annual meetings of the APA are held in September. An attempt is made to choose meeting sites in such a way that attendance will be convenient for members and their guests in different sections of the country at different times. Thus, the 1951 meeting will be held from August 31 through September 5 in Chicago, and the 1952 meeting in Washington, D. C. These meetings provide the members with an opportunity to present or hear reports of psychological research and lectures on topics of psychological interest, and to participate in formal and informal discussion groups. The research reports, lectures, and discussion groups cover varied topics of current interest within the field of psychology.

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The APA maintains an office in Washington which acts as a coordinating center for all APA activities. Journal subscriptions, membership applications, personnel placement requests, and general APA business procedures are handled in this office.

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UNIVERSITY MICROFILMS
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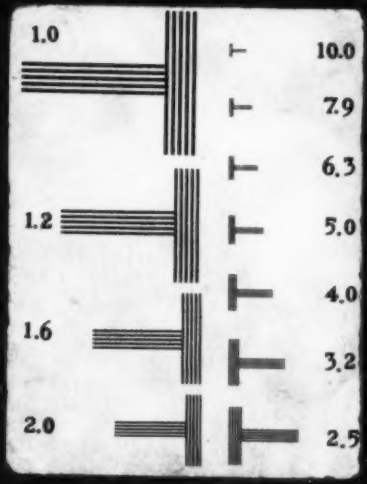
RESOLUTION CHART



100 MILLIMETERS

INSTRUCTIONS Resolution is expressed in terms of the lines per millimeter recorded by a particular film under specified conditions. Numerals in chart indicate the number of lines per millimeter in adjacent "T-shaped" groupings.

In microfilming, it is necessary to determine the reduction ratio and multiply the number of lines in the chart by this value to find the number of lines recorded by the film. As an aid in determining the reduction ratio, the line above is 100 millimeters in length. Measuring this line in the film image and dividing the length into 100 gives the reduction ratio. Example: the line is 20 mm. long in the film image, and $100/20 = 5$.

Examine "T-shaped" line groupings in the film with microscope, and note the number adjacent to finest lines recorded sharply and distinctly. Multiply this number by the reduction factor to obtain resolving power in lines per millimeter. Example: 7.9 group of lines is clearly recorded while lines in the 10.0 group are not distinctly separated. Reduction ratio is 5, and $7.9 \times 5 = 39.5$ lines per millimeter recorded satisfactorily. $10.0 \times 5 = 50$ lines per millimeter which are not recorded satisfactorily. Under the particular conditions, maximum resolution is between 39.5 and 50 lines per millimeter.

Resolution, as measured on the film, is a test of the entire photographic system, including lens, exposure, processing, and other factors. These rarely utilize maximum resolution of the film. Vibrations during exposure, lack of critical focus, and exposures yielding very dense negatives are to be avoided.

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